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e Making of Modern Science:
ographical Studies

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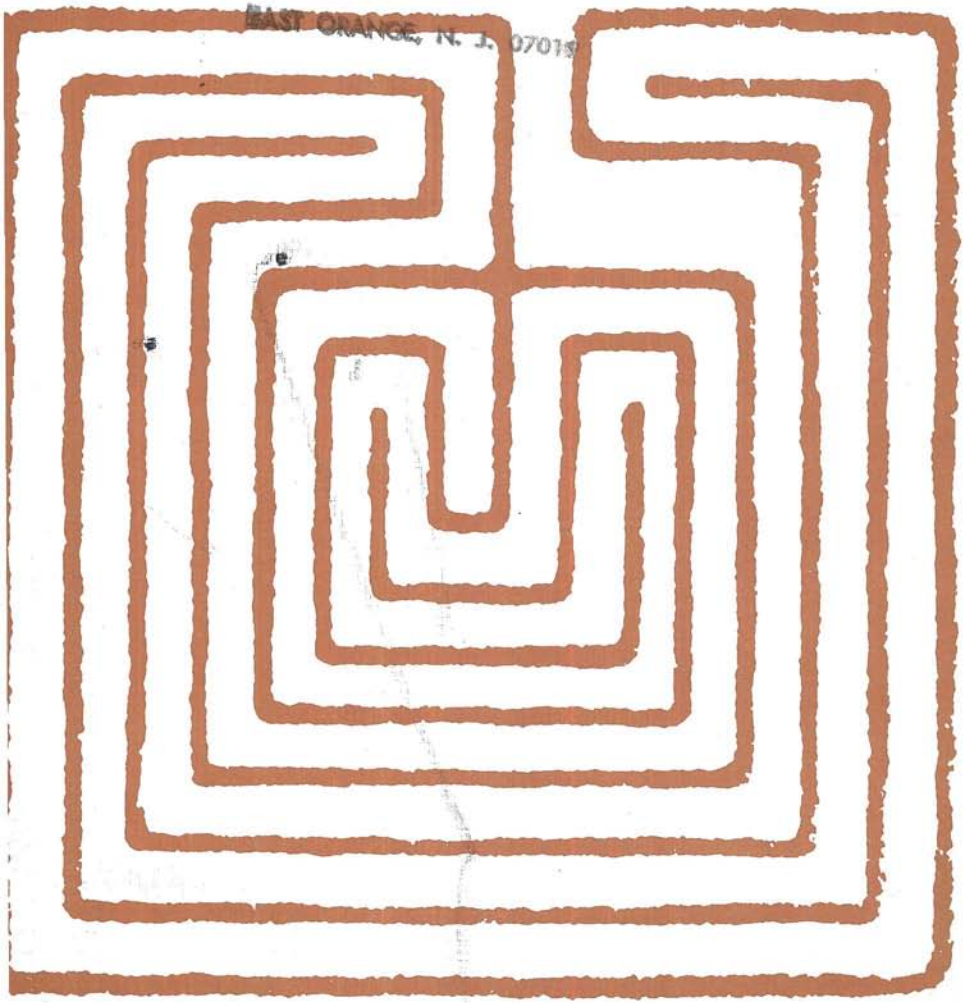

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GERALD HOLTON

Introduction to the Issue "The Making of Modern Science: Biographical Studies"

For a collection of essays such as this, the introduction should not be a roadblock before the essays themselves. On the contrary, all I really need say is that the essays speak best for themselves, and so there is little reason to delay reading them.

The general area covered here is not new to *Dædalus*. The topic of the first issue of this quarterly, which appeared about twelve years ago, was "Science and the Modern World View." Since then *Dædalus* has returned on several occasions to studies of the relation of the sciences to other fields. But this issue is the first to be devoted chiefly to the intellectual biography of modern sciences and scientists themselves, some of the chief developments and developers that have profoundly shaped and changed twentieth-century thought in science, and with it the whole conception of nature and man.

The chief reason for planning this issue is the existence of a paradoxical situation that should be attacked head-on. It is often acknowledged that our age has been transformed by major ideas of modern science—for example, those in quantum mechanics and genetics, in psychology and sociology—as well as by the organization of scientific research. What scientists have been doing at the frontiers of research has helped define what is modern about our time, and figures among its chief excitements. Indeed their work increasingly interests a number of fairly new professions—the sociologists of science, the philosophers of science, the historians of science, the new group interested in science and public policy. And yet, large numbers of intellectuals find they do not have sufficient access to the intellectual biography of scientific ideas.

We therefore urged our contributors to adopt any style congenial to them, but to express in each case the sense of science as a developing, sometimes inchoate private achievement that may have deeply moving consequences, rather than to stress only the more usual sense of science as a finished institutional product, as embalmed in textbooks, in dry

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success-story terms without a hint of its effects on other fields of thought or activity.

Taken as a whole the collection will, I think, meet this primary aim. While none of the essays is intended to replace an introductory textbook on the subject, they should help to provide the reader with an explanation of why a particular contribution became a major theme of twentieth-century thought. At the same time they also speak of origins (including those which may have been outside the sciences as narrowly defined); of influences of teachers and colleagues; of failures and unrealized ambitions; and of unforeseen effects that shaped the final outcome. In some cases, undoubtedly, a nonspecialist will have to take more of the technical material on faith than he would perhaps want. But at the authors' conference in Bellagio when the objection was raised that without understanding the technical details one could not follow a paper (on quantum mechanics, for example), a professor of zoology summed up the consensus when he exclaimed that he did not agree. Enough could be appreciated without going through the hopeless task of providing also a technical primer: "I am not a physicist either, and I have great difficulties in understanding some of this. But I felt somewhat as though I was in a Kabuki theater, entranced by the motion and action and highlights and excitement, and the lack of a technical background was not so serious that I could not understand what was going on." In spite of some technical difficulties, in most cases there is gained a basic authoritative document which only that particular author could have written, and to which scholars may continue to refer in the distant future.

While some of the essays are first-hand accounts of the "nascent moment," others give a wider, analytic view of the transforming influence of a conception, usually showing the most heroic phase of the rise of a new scientific field. This was intended in the design of the issue. Among the many possibilities clamoring for attention, we selected a few chief areas in which there is some degree of overlap. Where it was possible within the limits of space we have tried to obtain an autobiography or biography of an outstanding individual whose contribution will be clearly identified with a major creative achievement of twentieth-century science, and also a second, related essay from a scholar who, in an analytic article, could provide background on the larger context surrounding the contribution. Thus (arranged in an order from the least to the most mathematical sciences), Erik Erikson's autobiographical study of his work in psychoanalysis is followed by the sociological essays of Edward Shils and Talcott Parsons; a pair of essays by Curt Stern and Gunther Stent on genetics leads into two essays by Robert Olby and Linus Pauling, covering related developments in molecular biology, with strong links to chemistry and physics in Pauling's paper. My own essay on the origins

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of the complementarity point of view in quantum mechanics (together with an earlier essay on the philosophical pilgrimage of Einstein, published in spring 1968 as an anticipation and announcement of the current issue) provides a bridge to the subject matter of Alvin Weinberg's and Robert Wilson's essays, the organization of scientific work in its typically twentieth-century modes—namely, large-scale and often interdisciplinary—and all the advantages and pitfalls of teamwork in science. Finally, W. O. Baker writes of the influence on science of both the computer and the cybernetic notions that it has helped to inject into our work and thought.

One regrets that this single issue could not also do justice to the number of other fields that fully deserve to be represented. For example, advances in this century in fields such as mathematics, economics, areas of psychology other than those discussed here, linguistics, cosmology, and anthropology certainly merit our attention. Then, too, there are important topics such as the effects scientists have had upon technology, including some of the pathologies and abuses of scientific knowledge. In our planning meetings we also thought of other ways of shaping the material: the biography of a laboratory during a seminal period, for example.

I myself regret most the absence here of essays in two areas: some of the more spectacular successes as well as disappointments in the medical sciences, and the whole field of ecology. There will, happily, be room for essays in some of these fields in the book version of this issue which, like all *Dædalus* hard-cover editions, should be ready within a year or so after publication of the journal. There may also be the opportunity to do justice to other fields in a future issue of the journal, if *Dædalus* returns to the subject presented here.

So much then for the primary purpose of the issue; but there are a number of secondary purposes. For example, a series of essays such as these provides a base for responding to the current cry, now almost a cliché, for the demonstration of the relevance of the sciences. There are at least four aspects to this search for relevance, and the present collection seems to me to address itself to all of them. In its most basic sense the relevance of the sciences is intellectual excitement that comes from understanding an impressive feat of human achievement; the sort of satisfaction that can be derived from the discovery of the structure of a molecule is as self-validating as the experience of discovering the internal structure of a major literary work. If we do not celebrate them in one field they will soon not be celebrated in any other, and a society without such celebrations is doomed to grow dull and decay. The joy of intellectual engagement in the deep understanding of phenomena of the material world, and the joy of discovering thereby the success of

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one's rational and intuitive faculties—these are surely among the most relevant activities that can be pursued.

Secondly, the sciences are relevant because of their influence on the intellectual and material climate of the time. Everyone is familiar with examples of such long-range effects as Newton's influence on the imagination of eighteenth-century poets and theologians, or the genetic role of Faraday's "toys" in the development of electric motors and generators and then in the growth of cities, once subways, tramways, and elevators could be built that run on electricity. But there is no doubt that the same long-range effects on social organizations and values will arise from the work of many of the scientists here discussed or, indeed, who are contributing essays to this issue.

The third response to the cry for relevance, and the most usual one, is to point to immediate benefits rather than the long-term ones. The intentional use of basic science "for the relief of man's estate," in the phrase of Francis Bacon (or to provide a quick "spin-off," to use NASA language) are indeed goals that sometimes involve science in its basic sense. But here a warning is in order. It would be quite wrong to settle merely for the assistance that, say, chemistry can give in solving problems such as atmospheric pollution. It would be wrong for two reasons. First of all there really is and need be relatively little connection between today's basic research and current technological advance. Only rarely is a fundamental discovery made consciously as a prelude to a major technical improvement. And, indeed, the relevance of science to a society demanding immediate benefits is best seen in a completely different way. Technological advance all too often brings with it major social problems that arise as unforeseen by-products, and these problems cannot be cured or even properly understood through existing scientific or technological or political means alone. Rather, such cures depend to a large extent on basic scientific advances that are yet to be made. To put it differently, at the heart of social problems created by technological advances is the *absence* of specific, basic scientific knowledge. This fact gives the sciences a whole new mandate and opens a new range of expectation for basic research.

Examples come readily to mind. It is quite customary to say that the population explosion is in part caused by the advance of medical science. But one can equally well claim that the population explosion is bound to overwhelm us precisely because we do not yet have sufficient knowledge in an area of pure science. That is to say, the complex problem of overpopulation is due in some degree to current ignorance of the basic process of conception—its biophysics, biochemistry, physiology; no wonder therefore that attempts at controlling population are so halting.

Similarly, the problem of bringing food to hungry people in arid

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lands that are near the sea is to a large extent political, as are most of the problems which science is said, in its recent progress, to have acerbated. But it is also a problem for basic science: before it is possible to design far more economical desalination plants, a more fundamental understanding of the structure of liquids—one of the much neglected problems in current physics and chemistry—and of the way materials move through membranes will be needed. This is one of those wonderfully complex areas of work in which research, to be effective, appears to be of necessity interdisciplinary and large-scale. Simply having learned to do team research on the scale of twentieth-century installations described in two of the articles may turn out to be half the battle.

Yet another response to the question of relevance may be just this: science is a style of life that, as several of the essays here show, has values that are to be jealously preserved if we intend to build bulwarks against a rising anti-intellectualism. An example that comes out of several presentations is the way, by demonstration of scientific talent, a certain "minority group" has broken through and found a welcome—not without its preliminary trials—in the more established community. The minority group I speak of is, of course, that made up of young people. A whole set of social inventions and devices operates in the life of science to recognize and reward talent early. There are in these essays some quite touching evidences of this sort of beneficent patronage.

To the three reasons for bringing together these essays—for their own sake in order that they be read by those who care to be literate about what is happening in the sciences; as a survey of certain chief areas in terms both of individual contributions and the progress of the field as a whole, with some attention to the points where such areas overlap; and as contributions toward the current discussion on the relevance of science—there is another yet to be added. These essays should, for some scholars, contain raw material for their work. During the past two decades historical scholarship in modern science has been dramatically increasing in quantity and quality. Historical, sociological, philosophical studies which use a scientific contribution as a jumping-off point are beginning to flourish. Yet clearly there is room for additional conceptions and new approaches to historiography—for example, above all in the detailed study of actual cases in the genesis of modern theory.

The modern scholar has moved away from the somewhat simplified view of the formulation of scientific theory that was lately dominant. He is likely to look at cases such as those presented here in terms of at least several components: the differences between the pursuit of science as a private activity (what Einstein called the "personal struggle"), and science as a public institution of sharable discourse; the intersect between the chronological development of a field of study and the

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chronological development of the contributor himself; such sociological questions as the role of colleagueship, of the formal or informal institutional setting within which an individual contribution was made, or of the resistance to new ideas; the epistemological and other elements of the intellectual inheritance of the creator of a new field; the basis for those quasiaesthetic choices that are often made in actual work (for example, when some ad hoc hypothesis is accepted or rejected with a remark such as that of P. A. M. Dirac: "a theory that has some mathematical beauty is more likely to be correct than an ugly one").

All these elements are in fact illuminated by passages in the essays that follow. But I would also draw attention to another component in the analysis of specific case histories. It is what can be called the thematic aspects: the unknown or unconfessed preconceptions and suppositions which are at the base of every major contribution. As several authors indicate, a number of the most active themata in current science are in fact inherited from earlier centuries. (This is not surprising since, regardless of their successes or failures, it seems that we rely on relatively few themata for most of the sciences.) Thus the efficacy of mathematics for the explanation of natural phenomena, and particularly the explanatory role associated with small integers (as in quantum physics or genetics) would not surprise a Pythagorean. It may remain eternally fascinating that the contrasting traits the early geneticist dealt with could be put in terms of ratios of small whole numbers. But one has the impression that it was almost unavoidable, given a prejudice in favor of quantification and particularly in favor of small whole numbers, that they would sooner or later pave the way for some forms of genetics—and for atomic physics. It was Niels Bohr himself who said, "This interpretation of the atomic number [as the number of orbital electrons] may be said to signify an important step toward the solution of one of the boldest dreams of natural science, namely, to build up an understanding of the regularities of nature upon the consideration of pure number."

Such nineteenth-century themes as evolution are of course still very much with us, and they have reached their most potent stage in our century. One thinks here of the evolutionary aspect of Erikson's life-cycle concept and indeed the possibility illuminated thereby that the thema of evolution in the first instance derives from the life-cycle perception. The taming of randomness appears in these essays in several guises, as does the search for symmetry. The struggle between holism and reductionism is not new either. An alternative to reductionism, namely the acceptance of dualities, is a chief theme that runs through at least two of the essays.

Indeed, what is perhaps most impressive is that the transforming conceptions of twentieth-century science are based on more sophisticated versions of previously established themata. That seems to be our strength

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as well as our limitation. It appears that the advances that have allowed modern scientists to deal with greater and greater complexity rely not so much on utterly novel conceptual tools at the level of preconception and fundamental thematic attitude, but rather on a higher order of intellectual daring.

As the materials here show, this spirit is in no danger of decline. On the contrary, given a sufficient level of opportunity and morale, one may confidently expect the depth and intensity of transforming achievements in the next decades to be at least equal to those in the period discussed in these articles.

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ERIK H. ERIKSON

Autobiographic Notes on the Identity Crisis

I HAVE been encouraged to put the concept of "identity crisis" in the center of an autobiographic essay. This, quite fittingly, raises the question as to what kind of identity this concept can claim in a discussion of transforming conceptions of modern science. Whether or not identity crisis is itself such a conception in my own field, psychoanalysis, is difficult to decide in the absence of a representative of my profession who might well assert that the concept is not, strictly speaking, psychoanalytic, because it deals with matters too close to the "social surface" to preserve the essence of depth psychology. I would, at any rate, prefer the more modest assumption that I have been invited to write this essay because *Dædalus*, on the basis of some related past symposia,[1] takes it for granted that the problem of psychosocial identity is relevant to the motivational nature of innovation and builds a bridge between psychoanalytic and other approaches to it.

That I have attempted to demonstrate this bridge in two studies dealing with the interrelation of the life histories and the historical period of great innovators[2] also seems to suggest to some that I should be willing to confess some of the possible reasons for my having been the person who, at a given time in his life and in the history of psychoanalysis, came to observe and to name something by now so self-evident as the identity crisis and to explain, in fact, why it now seems so self-evident. For identity concepts have immediately secured for themselves, if not the clear status of scientific innovation, yet the onus of a certain novelty in the thinking or, at any rate, the vocabulary, of a wide range of persons in many countries. In my recent collection of essays on the subject[3] I reported fondly that not long ago a Catholic student organization at Harvard announced in the paper that it would "hold an identity crisis" in a given place on a certain evening at 8:00 P.M. sharp. More recently, it was reported that President Nixon's son-in-law, being also President Eisenhower's grandson, on that very score felt an identity crisis coming on, whereupon he consulted his father-in-law who in turn asked a

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minister to discuss the matter in a Sunday sermon at the White House. Finally, to continue on this high level, the Pope in a recent speech recommended the image of a newly sainted sixteenth-century Spaniard of Jewish descent to the young priests of our time—"a time when they say the priesthood itself suffers . . . a crisis of identity." The varied uses of the term naturally suggest, to many serious workers, a popularization beyond redemption, while I, of course, continue to insist that the term and what it really stands for is firmly anchored in conceptual necessities. Looking around for some confirmation, I can point to the fact that the items "Identity, psychosocial," and "Lifecycle, human" have appeared in the newest edition of the *Encyclopedia of the Social Sciences*.[4]

But as to the suggestion that I should go further and demonstrate how I would use my own tools in an attempt to look at my own life, let me say from the onset that I do not feel obliged to be as effusive in my self-revelations as were the two religious activists who are the subjects of my psychoanalytic studies. They, of their own volition, left passionate confessional data behind them, and this apparently as part of what they considered their over-all mission. Their confessions of overwhelming experiences and of singular usurpations "call" for the enlightened interpretation of subsequent generations. We, on the other hand, have prepared ourselves for just such a job by undergoing didactic psychoanalyses which cooled our self-revelatory fervor and made it clear to us that all voluntary confession can also serve both as ingenious cover up and as disarming propaganda: any "true" confession quickly leads to the unspeakable. My position, then, seems to be midway between the effusive subjects of my biographic efforts, and the representatives of the natural and social sciences in this volume who are reluctant to use sentences beginning with "I." My job is merely to demonstrate some motivational dimensions in the total life situation of an individual who comes to formulate something "new."

Let me present, at this point, a kind of glossary which will, if not define, at least circumscribe what an identity crisis *is*. Here I take heart from the reassurance of Stuart Hampshire, who states approvingly that I "leave my much misused concept of identity undefined" because it primarily "serves to group together a range of phenomena which could profitably be investigated together."[5] He understood, it seems, the difficulty of establishing the nature and the position of something that is *psycho* and *social*. For we have as yet no one social science comparable to the natural variety. In each of the social sciences, in fact, the workings of identity appear in different contexts of verifiability. To say then that the identity crisis is psycho *and* social means, when approached psychoanalytically, that the "psycho" side of it:

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1. Is partially *conscious* and partially *unconscious*. It is a *sense* of personal continuity and sameness, but it is also a *quality* of unself-conscious living, as can be so gloriously obvious in a young person who has found himself as he has found his communality. In him we see emerge a unification of what is irreversibly given (that is, body type and temperament, giftedness and vulnerability, infantile models and ingrained prejudices) with the open choices provided (available roles, occupational possibilities, values offered, friendships made, sexual encounters), and all this within traditional or emerging cultural and historical patterns.

2. It is beset with the dynamics of *conflict*, and especially at its climax can lead to contradictory mental states such as a sense of aggravated vulnerability and, alternatively, one of grand individual promise.

3. It has its own *developmental period*, before which it could not come to a crisis, because the somatic, cognitive, and social preconditions are not yet given; and beyond which it must not be delayed, because the next and all future developments depend on it. This developmental period is, of course, *adolescence and youth*, which also means that the identity crisis partially depends on *psychobiological* factors, which secure the somatic basis for an organism's coherent sense of vital selfhood.

4. It reaches both into the *past* and toward the *future*: it is grounded in the stages of childhood and will depend for its preservation and renewal on each subsequent stage of life.

The "socio" part of identity, on the other hand, must be accounted for in that communality within which an individual must find himself. No ego is an island to itself. Throughout life the establishment and maintenance of that strength which can reconcile discontinuities and ambiguities depends on the support first of parental and then of communal models. Youth, in particular, depends on the ideological coherence of the world it is meant to take over, and therefore is highly aware of whether the system is strong enough in its traditional form to be "confirmed" by the identity process, or sufficiently weakened to suggest renovation, reformation, or revolution. Psychosocial identity, then, also has a *psychohistorical* side, and life histories are inextricably interwoven with history.

All this sounds probable enough and, especially when shorn of its unconscious dimension, appears to be widely and faddishly acceptable in our day. The unconscious complexities usually ignored are outlined in my encyclopedia article, from which I paraphrase:

1. The crisis is sometimes hardly noticeable and sometimes very much so: in some young people, in some classes, at some periods in history, the identity crisis will be noiseless; in other people, classes, and periods, the crisis will be clearly marked off as a critical period, a kind of "second birth," institutionalized by ceremonial procedure, or intensified by collective strife or individual conflict.

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2. Identity formation normatively has its negative side which throughout life can remain an unruly part of the total identity. The *negative identity* is the sum of all those identifications and identity fragments which the individual had to submerge in himself as undesirable or irreconcilable or by which atypical individuals and marked minorities are made to feel "different." In the event of aggravated crises, an individual (or, indeed, a group) may despair of the ability to contain these negative elements in a positive identity. A specific rage can be aroused wherever identity development loses the promise of a traditionally assured wholeness: thus an as yet uncommitted delinquent may become a criminal. Such potential rage is also easily exploited by psychopathic leaders, who become the models of a sudden surrender to total doctrines and dogmas in which the negative identity becomes the dominant one: the Nazis fanatically cultivated what the victorious West as well as the refined Germans had come to decry as "typically German." The rage aroused by threatened identity loss can explode in the arbitrary destructiveness of mobs, or it can serve the efficient violence of organized machines of destruction.

3. The nature of the identity conflict often depends on the latent panic pervading a historical period. Some periods in history become identity vacua caused by three basic forms of human apprehension: *fears* aroused by new facts, such as discoveries and inventions (including weapons) which radically expand and change the whole world-image; *anxieties* aroused by symbolic dangers vaguely perceived as a consequence of the decay of existing ideologies; and the *dread* of an existential abyss devoid of spiritual meaning.

But why would some decisive insights concerning these universal matters first come from psychoanalysis? The fact is that psychoanalysis as a clinical science discovers new aspects of man's nature by trying to cure previously obscure disorders which, at a given time, suddenly seem to assume epidemiological significance—as did hysteria in Freud's younger years. In our time a state of *identity confusion*, not abnormal in itself, often seems to be accompanied by all the neurotic or near-psychotic symptoms to which the person is prone on the basis of constitution, early fate, and malignant circumstance. In fact, young individuals undergoing such a confusion are subject to a more malignant disturbance than might have manifested itself during the rest of their lives, because it is a characteristic of the adolescent process that the individual should semideliberately give in to some of his most regressed or repressed tendencies in order, as it were, to test rock bottom and to recover some of his as yet undeveloped childhood strengths. Clinically speaking, it is important to recognize that many young people have in the past been judged to suffer from a chronic disturbance, where a severe

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developmental crisis was indicated; while the epidemiological variations of such crises over the decades strongly suggest some relation to changing history. This, then, is the clinical anchorage for the conception of an identity crisis. To make the emergence of this conception plausible in historical and autobiographic terms I must now account for my clinical training, and then for the years of childhood and youth that preceded it.

I am about two years younger than the twentieth century, and I therefore can speak roughly of the decades of my life as coinciding with those of the calendar. I was graduated from the Vienna Psychoanalytic Institute in my (and the century's) early thirties.

The very beginning of my career marks me as one of those early workers in my field who had quite heterogeneous professional origins. I was an artist before I studied psychoanalysis, and can otherwise boast only of a Montessori diploma. In fact, if William James could say that the first lecture in psychology he ever took was the first one he gave, I must concede that the first course in psychology I ever took was also the first and the last I flunked. But, for reasons to be given presently, I seemed acceptable to the Vienna Psychoanalytic Institute, which was the training arm of a private society not connected with and often opposed and belittled by both academic departments and professional organizations. Although Sigmund Freud was a doctor and medical school lecturer, he had taken it upon himself to select the original circle of men and women (medically trained, as most were, or not) who were willing to learn his methods according to his prescription. For this they had to be gifted with a certain rare perceptiveness for the nonrational, and both sane and just a bit insane enough to *want* to study it. For them, Freud created simultaneously a training institute, a clientele, a publishing house, and, of course, a new professional identity.

The truly transforming scientific and therapeutic orientation created by Freud was based on a radical change in the role of the doctor as well as that of the patient, and thus in the nature of the clinical laboratory. If the "classical" hysterical patients, as Freud had concluded, were far from being the degenerates they were judged to be by his medical contemporaries, he could only conclude that the authoritative methods used to cure them violated, in fact, what alone can free a man from inner bondage, namely, the conscious acceptance of certain truths about himself and others. He advocated that the psychoanalytic practitioner himself undergo treatment so as to come to terms with his own unconscious and to acquire the capacity to explain, and not to evade or to condemn. The patient in turn was asked to verbalize all his available thought processes, and thus to become co-observer as well as client; while the analyst, as observer, continued to observe himself as he ob-

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served the patient's trend of thought. They were thus to become collaborators in the job of becoming conscious of (and of classifying) that reservoir of unconscious imagery and affect which soon proved not only to be festering in individual patients, but also to have been repressed in all past history by the whole race—except for occasional seers and prophets, creative writers and philosophers. The atmosphere thus created in the Vienna circle was one of intense mutual loyalty and of a deep devotion to a truly liberating idea, if often also of morbid ambivalencies and of deep and unforeseeable mental upset. Here much fascinating material is awaiting the historian who manages to become a historian of ideas and to resist the temptation merely to turn early psychoanalysis upon itself.

My own training psychoanalysis was conducted by Anna Freud, who accepted me as a fellowship candidate, and this on the basis of the fact that she and her friends had approvingly witnessed my work with children both as a private tutor and as a teacher in a small private school. Anna Freud had founded the Vienna version of the subspecialty of child analysis, and I, too, was to be primarily trained as a psychoanalyst of children, although the general training program included the treatment of adolescent patients and of adults. The reader will appreciate the complex feelings aroused by the fact that my psychoanalyst was the daughter of the then already mythical founder who often appeared in the door of their common waiting room, in order to invite his analysand into his study. But this is only one circumstance within the peculiar bonds and burdens of a training psychoanalysis. While such a preparation is of the essence in this kind of work, and while this essence can up to a point be comprehended in a systematic and disciplined way, it stands to reason that the generational succession of teachers, so important in all fields, will remain a special burden in the life of any maturing and even of the trained psychoanalyst—a burden reaching well into the conceptualizations he may later feel called upon to confirm or to disavow. Even the most venerable training psychoanalyst is (and must be) both welcomed as a liberating agent and resisted as a potential indoctrinator, and thus be both accepted and rejected as an identity model: for in all pursuits which attempt to gain a rational foothold in man's pervasive irrationality, insights retain an unconscious involvement which can only be clarified by lifelong maturation. At the same time, however, only the systematic clinical acquaintance with the unconscious can convey a certain ceaseless surprise over the creative order that governs affects never faced before and a certain sense of liberated sanity just because of the chaos faced. This is probably hard to comprehend except through the experience itself; and yet some of it must be visible in the impact of psychoanalysis on other fields and

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the augmented access it has provided to a wealth of data previously not visualized or not seen in their relation to each other. Considering the total newness of such data, it should not be surprising that it will take many generations to find the proper forms of verification and suitable methods of application: in the meantime, it may be possible also to establish analogies between this novel training situation, extreme in what we may call a *disciplined subjectivity,* and latent factors in more objective courses of study.

I should add here that during the years of my training Freud no longer taught and never appeared at public functions. I sometimes met him in his or the Burlingham's house or garden, but never addressed him, not only out of shyness but because of the pain that all attempts at speaking could cause him. He was in his early seventies; a radical upper-jaw operation had done away with a cancerous affliction years before, but the "infernal prothesis" which covered the roof of his mouth led to repeated fresh outbreaks and fresh operations. His daughter was also his nurse and his private secretary, his companion and his ambassador both to the old guard of the psychoanalytic movement and to the public on rare ceremonial occasions. Some of the writings which Freud published during these years were of a markedly philosophical bent, or what the Germans call *Weltanschaulich.* In his *Autobiographic Study,*[6] which he wrote in his sixty-eighth year, he himself ascribed this trend to "a phase of regressive development" back to his premedical and, in fact, adolescent interest in problems of culture, or rather *Kultur.* Theoretically, it all culminated in the concept of the Death Instinct—a grandiose contradiction in terms.

In the meantime, our training continued to impress on us in all their clinical immediacy the five conceptions which Freud in the same autobiographic study had called "the principal constituents of psychoanalysis." These five have remained fundamental to the modifications of psychoanalytic technique and to its application to other fields throughout this century. The most fundamental is "inner resistance," a term which was never meant to be judgmental, but physicalistic: all matter offers resistance to the energy which passes through it. And so, memories and thoughts are resisted even by him who wants to recover them, whether out of despair or out of curiosity. For such inner resistance, Freud blamed "repression," an anticonscious and defensive quality of the mind which marks the "unconscious" as much more than *not* conscious. Its drives and wishes, memories and phantasies, however, reassert their right to awareness and resolution, if only in indirect ways: in the symbolic disguise of dreams and day dreams, in symptoms of commission (acts alien to the actor or unintended in their consequences), and in symptoms of omission (inhibitions, avoidances, and so forth). If Freud,

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on the basis of his Victorian data, found in his patients' special repressions and resistance primarily what he called the "aetiological significance of sexual life"—that is, the pathogenic power of repressed sexual impulses—he, of course, called sexual a wide assortment of impulses and affects never previously included in that definition. It was the burden of the libido theory to show how much in life is codetermined by derivatives of the previously unrecognized infantile sexuality. Freud, therefore, considered systematic attention to the "importance of infantile experiences" an intrinsic part of his method and his theory: and we know now how his and Karl Abraham's first crude findings opened up a whole new view of the stages of life.

I would add to these five points the prime importance of what Freud calls "transference"—that is, a universal tendency to experience another person (unconsciously, of course) as comparable to an important figure of the preadult past. Such transference serves the inadvertent reenactment of infantile and juvenile wishes and fears, hopes and apprehension; and this always with a bewildering "ambivalence"—that is, a ratio of loving and hateful tendencies which under certain conditions coincide dangerously or alternate radically.

Transference, of course, plays a singularly important role in the clinical encounter; but it must be clear that all these interpersonal tendencies, which in the training analysis and in clinical work move into the center of attention, also exist in daily life, and especially where work arrangements are the basis for intense experiences of leader- and followerships and of fraternal or sororal rivalry. The question as to where and when such tensions support or hinder the inventiveness, solidarity, and altruism demanded of scientific workers has been asked repeatedly in these discussions. The answer is that mutual emotional involvement, even of the kind called transference, can evoke, under favorable conditions, powers of filial loyalty and sharpen issues of ambivalence, and can thus support personal growth and creative innovation. If psychoanalysis uses this power (which David McClelland unhesitatingly calls religious) for therapeutic purposes, it only puts to systematic use what Mircea Eliade[7] has described in primitive rites which seek rebirth in a return to origins: and Eliade rightly recognized a parallel in the (often quite ritualized) faith on which modern psychotherapy was founded.

This, however, does not justify the frequent assumption that psychoanalysis is primarily faith healing. To Freud and his followers the psychoanalyst's office has always been not only a healer's den, but also a psychologist's laboratory. What was thus observed had to find a classification, a terminology, and a methodology which would make therapeutic techniques ever more adequate in dealing with a widening range of pathological conditions and would help formulate a body of insights

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amounting to a communicable "field." That in some of Freud's most gifted followers idiosyncratic gifts as well as ideological predilections often seemed to obscure the ground plan for which he thought he alone had a firm sense of schedule—that is certainly not surprising. A truly historical study of these developments is probably far off; but it could throw much light on the influence of such personal passions and such residual pathology as are aroused when man's central motivations are submitted to observation and conceptualization. Yet it sometimes seems that such passions are never totally absent even from the most controlled laboratory work, even when assumptions that have contributed to the stability of a classical world-image are being questioned, and when the necessity of transforming ideas becomes a matter of competition between individuals and schools.

At that time, in Vienna, all theoretical training took place in the evenings in what today might be called something like a free psychiatric university. Again, only those who have attended similarly independent study groups of men and women serving what is felt to be a truly transforming idea—and serving at the sacrifice of income, professional status, and mental peace—will know of the devotional atmosphere in which (as every reader of Freud's work knows) no clinical detail was too small and no theoretical insight too big to merit intensive presentation and debate.

Psychoanalytic writings do not always reflect the high degree of medical and scientific common sense and humane humor pervading the actual study of ongoing treatments. The clinical laboratory includes the regular and exhaustive comparison of treatment histories. Clinical conferences, close to the data, are the heart of the matter. Freud's method, in fact, forced clinicians to attempt to "locate" any given clinical observation on a number of coordinates which he called "points of view." I have discussed these in detail in my paper on clinical evidence.[8] Here I can only indicate that these points of view include a "structural" one which would locate a given item in a model of the mind, the main compartments of which are the "id" (a cauldron of primeval drives and primal wishes), the "ego" (the organization of functions mediating between the id and the outerworld), and the "super-ego" (the internalization of standards which guard the ego's mediation). There is also a "dynamic" point of view, which takes account of the tension and conflict between these inner domains. An "economic" point of view, in turn, attempts to conceptualize the householding of energy in man's precarious inner balance. Finally, the "genetic" point of view permits the reconstruction of the origin and the development of all these structures, functions, and energies.

Although Freud called these points of view his "metapsychology,"

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and assigned to them a level of abstraction not accessible to direct observation, it is hard to overcome the impression that these points of view served as the bridge by which Freud, a fervent and painstaking medical researcher before he became the first psychoanalyst, could apply his anatomical, physiological, pathological, and developmental modes of thinking to the workings of the mind. I do not mean here Freud's early attempts, in tune with the dominant neurology of his medical school years, to link mental associations to concrete processes in the nervous system. These observations only served to open for him vast promises of finding by a new clinical method an approach to the very secrets of man's conflicted nature—and of making these secrets accessible to science. But as though the elemental sweep of the resulting discoveries had aroused a sense of hybris in the medical researcher he seems to have determined, in his metapsychology, to reconnect his vast findings with the disciplined thought patterns which in his young manhood had commanded his fidelity and helped establish his occupational identity. It is often overlooked that even his preoccupation with an all-pervasive libido was related to a scientific commitment to think in terms of energies *"equal in dignity"* to the forces found in physics and in chemistry. On their neurological homeground, however, such modes of thinking had been based on visible, observable, and verifiable facts, while in the study of the mind they sooner or later served, especially in the hands of dogmatic followers, as unchecked reifications—as though the "libido" or the "ego" had, after all, become observable entities.

The intellectual milieu governing the many evenings spent in small, intensive seminars and "continuous case" discussions (and some were so small that we could comfortably meet in our teachers' homes) is best characterized by a listing of these teachers. All but the first two later came to this country to preside over the strange fate of a psychoanalysis, influential in medical training, lucrative in practice, and popularized in the media.

My training in child analysis took place in the famous *Kinderseminar* led by Anna Freud; that in the treatment of juvenile disturbances (including delinquency) was directed by August Aichhorn. Heinz Hartmann was the leading theoretician, and his thinking which later culminated in his monograph on the adaptive function of the ego influenced me deeply. The basic theoretical struggle at the time was between Anna Freud's clarification of the defensive mechanisms employed by the ego against the drives[9] and of Hartmann's explorations of the ego's adaptive response to the environment.[10] One of the most obscure and yet fascinating teachers was Paul Federn, and it is quite possible that in his seminar I first heard the term identity mentioned in one of its earlier usages. The preoccupation with the ego was then replacing that earlier pansexual at-

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tention to the id which was based on Freud's original determination to find the whole extent of man's enslavement to sexuality. What survived from that first period was a strangely ascetic veneration of Eros, a totally intellectual bacchanalia. Nothing seemed to be further from the mind of those early workers (not even the followers of Wilhelm Reich) than that psychoanalysis might someday be used as an argument for sexual freedom outside the rules of a bourgeois or, for that matter, a proletarian convention. Both Reich and, as visitor, Siegfried Bernfeld, who was deeply involved in problems of youth, I remember as very inspiring teachers already then driven to a certain tragic isolation by their belief that Freud's "libido," which sounded so tangibly quantitative, would *have* to be found and isolated physically. In fact, the student could not help sensing in the didactic milieu a growing conservatism and especially a pervasive interdiction of certain trends of thought. This concerned primarily any idea which might be reminiscent of the deviations perpetrated by those earliest and most brilliant of Freud's co-workers (such as Rank, Adler, and Jung) who had been separated from the movement already before World War I. In other words, the psychoanalytic movement was now already working under the impact of its own historical trauma, its own rebellion against the founder. The possible merits and decisive demerits of those deviations the student could not judge. I must admit that, after such intense training under such complex conditions, the idea of moving on and working independently seemed an invigorating idea. Vienna, at that time, chose not to foresee the total disruption that would soon separate the regions of Europe, not to speak of the old country and the New World.

If I should now briefly indicate what uncertainty and curiosity I took with me when my graduation coincided with my emigration from Europe, I would oversimplify it in the following way. Psychoanalysis had broken through to much that had been totally neglected or denied in all previous models of man: it had turned *inward* to open up man's inner world to systematic study; it searched *backwards* to the ontogenetic origins of the mind and of its disturbances; and it pressed *downwards* into those instinctual tendencies which man thought he had overcome when he had repressed or denied the infancy of individuals and the evolution of the race. That (as Darwin, too, had discovered) was the territory to be conquered, the origins to be acknowledged. But conquerors so easily lose themselves in the discoveries of the new territory; how to reassimilate them to what is already known—that is the job of the second stage. The question remained, I felt dimly, whether an image of man reconstructed primarily on the basis of observation in the clinical laboratory might not lack what, in man's total existence, leads *outward* from self-centeredness to the mutuality of love and communality, *for-*

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ward from the enslaving past to the utopian anticipation of new potentialities, and *upwards* from the unconscious to the enigma of consciousness. All of this, however, seemed to me always implicit in Freud's own writings: if not in the *content* he grimly pursued, then in the grand *style* of this pursuit—the style for which, in those very days, he received the Goethe Prize as the best scientific writer in the German language. To Freud, the *via regna* to mental life had been the dream. For me, children's play became the first *via regna* to an understanding of growing man's conflicts and triumphs, his repetitive working through of the past and his creative self-renewal in truly playful moments. I identified with Freud, then, not as the former laboratory worker who insisted on a terminology made for the observation of transformable quantities of drive enlivening inner structures, but as the sharp discerner of verbal and visual configurations which revealed what was clearly suggested or flagrantly omitted, that meant what it said or meant the opposite. To put it bluntly, I have always suspected (maybe because I do not really understand these things) that what sounded most scientific in psychoanalysis in terms of nineteenth-century physicalism was more scientism than science, even though I understood that psychology and social science in attempting to free themselves from philosophy and theology had no choice but to try, for a while, to think in the scientific imagery of the century. But Freud's phenomenological and literary approach, which seemed to reflect the very creativity of the unconscious, held in itself a promise without which psychoanalytic theory would have meant little to me. This may be one reason why, in later years, I proved inept in theoretical discussion and was apt to neglect ruefully the work of my colleagues—and not only where they seemed to take Freud at his most atomistic and mechanical word, or where they turned neo-Freudian. All this may well have an admixture of a particular transference on Freud. But then, he was the father of it all—a fact which I probably tried to objectify in my later studies of great men, as well as in a few essays on Freud himself.[11]

But before I sketch the direction of my own work which, in a new era and on a new continent, brought me to such concepts as the "identity crisis," I will briefly account for those aspects of my childhood and youth which had led me to Vienna and then pursued me to this country.

In the Europe of my youth the choice of the occupational identity of "artist" meant, for many, a way of life rather than a way of making a living; and, as today, it could mean primarily an anti-Establishment way of life. Except that the European Establishment had created a well-institutionalized social niche for such idiosyncratic needs. A certain adolescent and neurotic shiftlessness could be contained in the custom

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of *Wanderschaft;* and if the individual had some gifts into the bargain, he could convince himself and others that he should have a chance to show that he might have a touch of genius. There were, of course, youth movements for those who wanted to abandon themselves to some collective utopia; but much of what young people today display in alienated and concerned groups was then more commonly experienced in an isolation shared only with a few equally special friends. To be an artist, then, meant to have at least a passing identity, and I had enough talent to consider it for a while an occupational one. The trouble was, I often had a kind of work disturbance and needed time. *Wanderschaft* under those conditions meant neurotic drivenness as well as deliberate search, even as today dropping out can be a time of tuning in, or of aimless negativism. But somehow, when we did not work, we had a deep and trusting relationship (often called "romantic" today) to what was still a peasants' Nature; we kept physically fit by interminable hiking; we trained our senses to changing perspectives, and our thoughts to distilled passages of, say, Angelus Silesius and Laotse, Nietzche and Schopenhauer, which we carried in our knapsacks. I will not describe the pathological side of my identity confusion which especially in its psychosexual aspects included disturbances for which psychoanalysis later seemed, indeed, the treatment of choice, while no doubt it assumed at times what some of us today would call a "borderline" character—that is, the borderline between neurosis and psychosis. But then, it is exactly this kind of diagnosis to which I later undertook to give a developmental perspective. And indeed, some of my friends will insist that I needed to name this crisis and to see it in everybody else in order to really come to terms with myself. And they can quote a whole roster of problems related to my *personal* identity.

There is first of all the question of origin which often looms large in persons who are driven to be original. I grew up in Karlsruhe in Baden as the son of a pediatrician, Dr. Theodor Homburger, and his wife Karla, née Abrahamsen, a native of Copenhagen, Denmark. All through my earlier childhood they kept secret from me the fact that my mother had been married previously and that I was the son of a Dane who had abandoned her before my birth. They apparently thought that such secretiveness was not only possible (because children then were not meant to know what they had not been told) but also advisable, so that I would feel thoroughly at home in their home. As children will do, I played in with this and more or less forgot the period before the age of three when my mother and I had lived alone. Then, her friends had been artists working in the folk style of Hans Thoma of the Black Forest. They, I believe, provided my first male imprinting before I had to come to terms with that intruder, the bearded doctor, with his healing

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love and mysterious instruments. Later, I enjoyed going back and forth between the painters' studios and our house, the first floor of which, in the afternoons, was filled with tense and trusting mothers and children. My sense of being "different" took refuge (as it is apt to do even in children without such acute life problems) in phantasies of how I, the son of much better parents, had been altogether a foundling. In the meantime, however, my adoptive father was anything but the proverbial stepfather. He had given me his last name (which I have retained as a middle name) and expected me to become a doctor like himself.

Identity problems sharpen with that turn in puberty when images of future roles become inescapable. My stepfather was the only professional man (and a highly respected one) in an intensely Jewish small bourgeoisie family, while I (coming from a racially mixed Scandinavian background) was blond and blue-eyed, and grew flagrantly tall. Before long, then, I acquired the nickname "goy" in my stepfather's temple; while to my schoolmates, I was a "Jew." Although I had tried desperately to be a good German chauvinist, I became a "Dane" when Denmark remained neutral during the First World War. The general setting was that of an old state capital of a Lutheran principality (we lived on the castle square) with a sizeable Catholic population. I do not remember having been interested in the old Luther of the state church or, for that matter, in the young one; and yet years later I was to choose young Luther as a matter of course for the presentation of some views on youth, history, and primal Christianity. At the time, like other youths with artistic or literary aspirations, I became intensely alienated from everything my bourgeois family stood for. At that point I *wanted* to be different. After graduation from the type of high school called a humanistic gymnasium (where one then acquired classical *Bildung* and a sense for languages), I went to art school, but always set out again to be a wandering artist—as described. I now consider those years an important part of my training. Sketching (as even a man like William James experienced) can be a good exercise in tracing impressions. And I enjoyed making very large woodprints: to cut stark images of nature on this primary material conveyed an elemental sense of both art and craft. And in those days every self-respecting stranger in his own (northern) culture, drifted sooner or later to Italy, where endless time was spent soaking up the southern sun and the ubiquitous sights with their grand blend of artifact and nature. But if this was a "moratorium," it certainly also was a period of total neglect of the military, political, and economic disasters then racking mankind: as long as one could expect some financial support from home, and was not suddenly lost in some cataclysm, one lived (or so one thought) by the measure of centuries, not of decades. Such narcissism obviously could be a young person's

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downfall unless he found an overweening idea and the stamina to work for it.

It was my friend Peter Blos (today a New York psychoanalyst best known for his classical writings on adolescence)[12] who came to my rescue. During our later childhood in Karlsruhe, he had shared his father with me, a doctor both prophetic and eccentric (he first told us about Gandhi), and we had been friends in Florence. Now he invited me, with the encouragement of the founder, Dorothy Burlingham, to join him in that small school in Vienna. With his help I learned to work regular hours, and I met the circle around Freud.

It must be more obvious now what Freud meant to me, although I would not have had words for it at the time. Here was a mythical figure and a great doctor who had rebelled against the medical profession. Here also was a circle which admitted me to the kind of training that came as close to the role of a children's doctor as one could possibly come without going to medical school. What, in me, responded to this situation was, I think, an ambivalent identification with my stepfather, the pediatrician, mixed with a search for my own mythical father. And if I ask myself in what spirit I accepted my truly astounding adoption by the Freudian circle, I can only surmise (not without embarrassment) that it was a kind of favored stepson identity that made me take for granted that I should be accepted where I did not quite belong. By the same token, however, I had to cultivate not-belonging and keep contact with the artist in me: my psychoanalytic identity therefore was not quite settled until much later, when with the help of my American wife, I became a writing psychoanalyst, if again in a language which had not been my own. At any rate, I could begin to repay my debt to the Freuds, too, only in my currency.

One could well suspect that later on I succeeded in making a professional life style out of being a stepson when I, throughout, worked in institutional contexts for which I did not have the usual credentials—except, of course, for my psychoanalytic training. But, as pointed out, psychoanalysis then almost methodically attracted and collected men and women who did not quite belong elsewhere, and some of my most outstanding colleagues have followed similarly irregular life plans. What is to be demonstrated here, then, is not singularity of achievement, but the configurational affinity of life plan and choice of concepts.

While I am on the stepson theme, I must also, with due surgical brevity, expose the dangers of such a development both for a person's character and his concepts. That a stepson's negative identity is that of a bastard need only be acknowledged here in passing. But a habitual stepson might also use his talents to avoid belonging anywhere quite irreversibly; working between the established fields can mean avoiding

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the disciplines necessary for any one field; and being enamored with the aesthetic order of things, one may well come to avoid their ethical and political as well as their conceptual implications. If one can find all these weaknesses in my work, there are also some energetic attempts to balance them, and this exactly in a serious and methodical turn to social and political conditions and—possibly inspired by my great compatriot Kierkegaard—to religious actualists such as Gandhi, certainly a man almost totally devoid of the aesthetic dimension.

That much about the circumstances of my life. If it seems obvious that such a life would predispose a person to a severe identity crisis, this must be said to be only partially true; for in my instance the more obvious identity conflicts concerned my *personal* identity and *psychosocial* choices which were relatively clearly delineated. If the malignancy of the identity crisis is determined both by defects in a person's early relationship to his mother and on the incompatibility or irrelevance of the values available in adolescence, I must say that I was fortunate in both respects. Even as I remember the mother of my early years as pervasively sad, I also visualize her as deeply involved in reading (what I later found to have been such authors as Brandes, Kierkegaard, Emerson) and I could never doubt that her ambitions for me transcended the conventions which she, nevertheless, faithfully served. On the other hand, she and my stepfather had the fortitude to let me find my way unhurriedly in a world which, for all the years of war and revolution, still seemed oriented toward traditional alternatives, so that the threatening cataclysms could still be ascribed to criminal men and evil nations or classes. What I eventually came to describe as more malignant forms of identity crises both in groups and in individuals was probably of quite a different order than what we then experienced. All the warring ideologies of our young years harbored some *saving* scheme which was to dominate forever after just one more war, just one more revolution, just one more new deal. It is only in our lifetime that the faith in change has gradually given way to a widespread fear of change itself—and a suspicion of faith itself. Identity problems and even the symptoms of identity confusion probably have changed accordingly. At any rate, the comparative study of the nature of identity crises at different periods of history (and in different groups during the same period) may well turn out to be a historical as well as a clinical tool, provided that the uses of the concept itself are submitted to historical scrutiny.

The direction of a man's work, then, always derives from history as well as from his life history. The German holocaust opened other countries to the migration of psychoanalysts who not only survived but, in fact, succeeded in establishing power spheres in the cities of their choice.

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In adapting to new classes of patients in a variety of national and cultural settings (and also by reflecting on their own fate and on the fate of those who had perished), some psychoanalysts found it mandatory to revise their model of human functioning.

My training psychoanalysis had ended when I met and married Joan Serson, then a dancer and teacher, later also an artist and writer; she, too, was a member of our small school. At about the time when Hitler came to power in Germany, I graduated and we left Vienna with our two small sons. I first attempted to regain my Danish citizenship and to help establish a psychoanalytic training center in Copenhagen. When this proved impracticable, we emigrated to the United States and settled in Boston where a psychoanalytic society had been founded the year before. Since my graduation in Vienna had made me a member of the International Psychoanalytic Association I was welcomed in the American Association as well. Although the medical professionalization of psychoanalysis in the United States would soon thereafter lead to the exclusion of further nonmedical candidates from the national association, I remained as one of its very few nonmedical members, hoping that the quiet contributions of nonphysicians would sooner or later impress American psychoanalysts with the wisdom of Freud's conviction that this field should not be entirely subordinated to medical professionalism. Personally I was, of course, ready to abide by the medical and legal cautions necessary in therapeutic work; and I cannot say that my being a nonphysician has ever interfered with my work.

In fact, to an immigrant with a specialty (and the term immigrant had not yet given way to that of refugee) this country proved, indeed, a land of unlimited possibilities. Harvard, and later Yale, did not hesitate to provide medical school appointments, and thus an expanded clinical experience. At Harvard there was also Harry Murray's Psychological Clinic, where an intensive study of students proved a valuable guide to the characteristics and values of American academic youth, while Murray's style of thinking conveyed something of the grand tradition of William James. And there was a flowering of interdisciplinary groups, led and financed by imaginative men like Lawrence K. Frank of the General Education Board and Frank Freemont-Smith of the Josiah Macy Jr. Foundation, and vigorously inspired by such wide-ranging observers as Margaret Mead and Kurt Lewin. Each participant was expected to make himself understood at these small and intense meetings, and I think that this also taught me (as I slowly learned to speak and write in English) to write for an interdisciplinary audience, an effort which, in turn, may have had some influence on my choice of concepts. After the Second World War, we made contact again with receptive colleagues from all over Europe in the Child Study Group of the World

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Health Organization. The conferences of the American Academy of Arts and Sciences have since assumed the important role of interdisciplinary meetings in my professional life.

In the thirties, I was first of all a practicing psychoanalyst, primarily with children, making frequent excursions to clinical conferences in the medical area of Harvard. I did some graduate work in psychology on the side, but when the Yale Medical School gave me a full-time research appointment, I decided to weather the future without belated degrees. The Yale Institute of Human Relations offered then a remarkable interdisciplinary stimulation under the leadership of John Dollard; and my job permitted me my first field trip (with Scudder Mekeel) to the Sioux Indians in South Dakota. I spent the forties in California, having been invited to abstract the ongoing records of a longitudinal study (led by Jean Macfarlane) of a cross section of Berkeley children. From there I made my second field trip (with Alfred Kroeber) to the Yurok Indians in California. Later, having been appointed a training psychoanalyst, I resumed private practice in San Francisco, but continued to act as a consultant in various public clinics, including a veteran rehabilitation clinic at the conclusion of the Second World War. My first professorship, in Berkeley, was short-lived because of the loyalty oath controversy during the McCarthy era. I was fired before the first year was up, and, after being reinstated as politically dependable, I resigned because of the firing of others who were not so judged. As I think back on that controversy now, it was a test of my American identity; for when we foreign born among the nonsigners were told to "go back where we came from," we suddenly felt quite certain that our apparent disloyalty to the soldiers in Korea was, in fact, quite in line with what they were said to be fighting for.

I think I have now said enough about myself to come to the question of how the concepts of "identity" and "identity crisis" emerged from my personal, clinical, and anthropological observations in the thirties and forties. I do not remember when I started to use these terms; they seemed naturally grounded in the experience of immigration and Americanization. As I summed the matter up in my first book which appeared in 1950:

> We began to conceptualize matters of identity at the very time in history when they become a problem. For we do so in a country which attempts to make a super-identity out of all the identities imported by its constituent immigrants; and we do so at a time when rapidly increasing industrialization threatens these essentially agrarian and patrician identities in their lands of origin as well.
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> The study of identity, then, becomes as strategic in our time as the study of sexuality was in Freud's time. Such historical relativity in the development

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of a field, however, does not seem to preclude consistency of ground plan and continued closeness to observable fact. Freud's findings regarding sexual etiology of a mental disturbance are as true for our patients as they were for his; while the burden of identity loss which stands out in our considerations probably burdened Freud's patients as well as ours, as re-interpretations would show. Different periods thus permit us to see in temporary exaggeration different aspects of an essentially inseparable whole.[13]

Identity problems were in the mental baggage of generations of new Americans, who left their motherlands and fatherlands behind to merge their ancestral identities in the common one of self-made men. Emigration can be a hard and heartless matter, in terms of what is abandoned in the old country and what is usurped in the new one. Migration means cruel survival in identity terms, too, for the very cataclysms in which millions perish open up new forms of identity to the survivors. In the Roosevelt era, we immigrants could tell ourselves that America was once more helping to save the Atlantic world from tyranny; and were we not hard at work as members of a helping profession which—beyond the living standards it made us accustomed to—contributed to a transforming enlightenment apt to diminish both the inner and the outer oppression of mankind? What now demanded to be conceptualized, however, called for a whole new orientation which fused a new world-image (and, in fact, a New World image) with traditional theoretical assumptions. I could not look at my patients' troubles any more in (what I later came to call) "originological" terms—that is, on the basis of where, when, and how "it all started." The question was also, where were they going from where they were and who was going with them? And if something like an identity crisis gradually appeared to be a normative problem in adolescence and youth, there also seemed to be enough of an adolescent in every American to suggest that in this country's history, fate had chosen to highlight identity questions together with a strangely adolescent style of adulthood—that is, one remaining expansively open for new roles and stances—in what at the time was called a "national character." This, incidentally, is not contradicted by the fact that today some young adults are forcefully questioning the nation as to what generations of Americans *have*, indeed, *made* of themselves by claiming so irreverently to be self-made, and what they have made of their continent, of their technology, and of the world under their influence. But this also means that problems of identity become urgent wherever Americanization spreads, and that some of the young, especially in Americanized countries, begin to take seriously not only the stance of self-made men, but also the question of adulthood, namely, how to *take care* of what is being appropriated in the establishment of an industrial identity.

At any rate, the variety of my clinical and "applied" observations now helped me to see a nexus of individual and history as well as of past and

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future: the Berkeley children, in the particular setting of their parents' Californization, could be seen approaching a special and yet also normative identity crisis, such as seemed to be built into the human life plan. A different version of such a crisis could be seen in the American Indians, whose expensive "reeducation" only made them fatalistically aware of the fact that they were denied both the right to remain themselves or to join America. I learned to see traumatically renewed identity crises in those returning veterans of World War II who had broken down with what were alternately called symptoms of shock or fatigue or of constitutional inferiority and malingering. In retrospect, I had by then recognized a national identity problem in that most surrounded great nation in Europe, once defeated and humiliated Germany, now hypnotized by a markedly adolescent leader promising a thousand years of unassailable super-identity. And I could later, in the fifties, verify the symptoms of acute and aggravated identity confusion on my clinical home ground—that is, in the young patients of the Austen Riggs Center in the Berkshires where I had turned after the University of California debacle. There also I found my critic and friend David Rapaport, who professed to see a place for my concepts (next to Heinz Hartmann's) in the edifice of psychoanalytic ego psychology,[14] but not without having added to the dynamic, structural, economic, and genetic points of view an "adaptive" one, relating the ego to the environment. In the sixties I temporarily suspended my clinical work in order to learn how to teach my whole conception of the life cycle—including the identity crisis—to people normatively very much in it: Harvard undergraduates. But this is another chapter, as is the more systematic pursuit of psychohistorical problems by myself and by my colleagues during this decade. In this essay, I restrict myself to the emergence of the identity concepts.

Clinical verification, as I have indicated, is always of the essence in any conceptual shift in psychoanalysis because it confirms that (and why) a syndrome such as "identity confusion" is not just a matter of contradictory self-images or aspirations, roles or opportunities, but a central disturbance dangerous for the whole ecological interaction of a mind organism with its "environment"; man's environment, after all, is the shared social universe. The symptoms of identity confusion, then, could be found in the psychosomatic sphere as well as in the psychosocial one, in the times as well as in the individual. Psychosocial identity proved to be "situated" in three orders in which man lives at all times.

1. The somatic order, by which an organism (as René Dubos has emphasized in our discussions) seeks to maintain its identity in a constant renewal of the mutual adaptation of the *milieu intérieur* and the environment.

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2. The ego order, that is, the integration of personal experience and behavior.

3. The social order maintained together by ego-organisms sharing a geographic-historical setting.

These three orders, however, are apt to endanger each other even as they are relative to each other. Much of man's creative tension as well as of his debilitating conflict originate in this incompatibility. For these orders seem to wholly support each other only in utopian schemes, which give man the visionary impetus to correct, at intervals, the accrued dangers to health, sanity, or social order. In this context, too, the study of the identity crisis in adolescence becomes strategic because at that stage of life, the organism is at the height of its vitality and potency; the ego must integrate new forms of intensive experience; and the social order must provide a renewed identity for its new members, in order to reaffirm—or to renew—its collective identity.

At this point, it must be rather obvious why the concept of identity crisis also helped me to recognize the transforming function of the "great man" at a certain junction of history. As I put it in my book on young Luther: deeply and pathologically upset, but possessed both by the vision of a new (or renewed) world-order and the need (and the gift) to transform masses of men, such a man makes his individual "patient-hood" representative of a universal one, and promises "to solve for all what he could not solve for himself alone."

Finally, after all the new insights that totalitarianism, nuclear warfare, and mass communication have forced us to face, it can no longer escape us that in all his past man has based much of his identity on mutually exclusive group identities in the form of tribes, nations, castes, religions, and so on. We really suffer from an evolutionary identity problem: is man one species, or is he destined to remain divided into what I have referred to as "pseudo-species" forever playing out one (necessarily incomplete) version of mankind against all the others until, in the glory of the nuclear age, one version will have the power and the luck to destroy all others just moments before it perishes itself?

If in a given field the "classical" approach is identified totally with one man, it stands to reason that in his lifetime or right after his death even the most necessary conceptual transformations become associated with emotions of sacrificial loyalty or, indeed, father murder. Thus, often quite in contrast to the founder's own irreverent beginnings, the followers' thinking comes under the shadow of the question of what "he" would have approved or would have disavowed, as if even the greatest among men could (if, indeed, they would) guarantee the purity of their

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creation once it enters the domain of historicity. Incidentally, to decide what the word "classical" really implies in all its logical and emotional ramifications would be well worth some psychohistorical study. In any given historical period there must be a mutual assimilation of all the concepts deemed classical in various fields; for even as the individual ego attempts to maintain a livable orientation in the multiplicity of experience, so must a civilization strive to receive and to integrate all transforming ideas into a coherent universe. Such a process is, of course, more obvious in religio-ideological and medico-humanitarian pursuits because they both simplify and unify in order to reassure, cure, and teach. But even in the hallowed objectivity of science, truly transforming ideas seem to have a fate which not only leads from scientific incredulity to verification, but also from philosophical repugnance to a new sense of classical coherence proving once more that "God does not play dice with mankind." And who could be greater, even if he is only very partially understood, than the modest scientist who takes a daring new look behind appearances and returns to affirm that God knows what He is doing?

And being somewhat of an expert in play I must add that while we remember great men usually as severe creators, they are certainly also the ones in whom a divine playfulness is undiminished in its capacity to transcend in new formulae some of the traumatic discrepancies of the times. To me, Freud, too, had throughout his decades played so sovereignly with so many types of conceptualization (from nature philosophy to economics) that I could not see how he could have thought of himself as anybody but a man who had to *bring the thought patterns of the millenia up to the moment of scientific introspection* while utilizing the modes of hippocratic observation which had become part of his medical identity. Perhaps great transforming ideas always contain only a limited number of truly provable assumptions—enough to establish some lasting roots in observation while branching out into new world-images.

Because of the marked ideological-humanitarian aspects of psychoanalysis, the development of a revolutionary approach into a classical and even an orthodox one—and into the resulting heresies—took little time. I, at least, could witness it in one lifetime; although I must admit that my primary interest in the flux of phenomena precluded any attempt to find safety in orthodoxy or escape in heresy.

As indicated, Freud had, to my mind, transferred the modes of medical research to psychology as he learned to "locate" early psychic traumata in a way analagous to the investigation of brain lesions. Such transfer of conceptual imagery could probably be demonstrated in the work of other originators of transforming ideas and found to be highly productive if balanced and counteracted by other modes of thought pervading the thinker's over-all identity. In Freud's case there was, no doubt, a con-

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tinuous struggle between the doctor role and such powerful identity fragments as liberator, scholar, writer. These he permitted himself to cultivate only sparingly, for he was, above all, a physician who would cure man of the curse of the past whether it originated in evolution, in primitive history, or in early life history—all three aspects of man's life discovered in Freud's very century by the Darwinists, the archeologists and anthropologists, and by himself: his many analogies between the mind's layers and archeological research come to mind here. Freud's antiteleological concern with the past as incapsulated and entombed in the mind has led, both in practice and theory, to astonishing discoveries; and if it is true that the mood of this kind of mastery of the past resembled somewhat the ritual reenactment of beginnings (creation, spring, birth), then it would seem plausible that psychoanalysis appealed above all to people who had lost their origins in soil, ritual, and tradition. But as we have seen, it will not do to explain human phenomena by their origins in childhood without asking why and how the social environment initiates, reinforces, and aggravates selected childhood conflicts and makes their outcome part of the positive and negative identity fragments which will vie with each other in adolescence. By an exclusive emphasis on origins, psychoanalysis has in fact contributed to a world-image pervaded by a new sense of predestination, which, it so often seems, can be alleviated only by a religious faith in psychoanalysis, or at least in its habitual vocabulary.

Freud's combination of a retrospective and introspective approach in psychoanalysis is often referred to as his "fatalism"; while I am only too aware of the fact that later concepts such as mine are welcomed by many as a more "optimistic" promise of life chances not doomed by childhood experience. But identity concepts only emphasize for one stage of life what is true for all, namely that periods of rapid growth and of a widening range of cognition permit, in interaction with living institutions, a renewal of old strengths as well as an initiation of new ones. That, however, does not in itself provide a more benign outlook. Rather it demands new and ruthless insights into the functioning of society and this especially in a world of rapid and unpredictable change.

Are such conceptions as the identity crisis, then, mere additions to the classical scheme, or do they call for a transformation in clinical and theoretical outlook? I can only introduce this question in conclusion with a few notes on what, over the years, I have found to be significant theoretical differences between the classical psychoanalytic outlook and newer perspectives such as my own. These differences will highlight what I meant when, earlier in this paper, I suggested that backward *and* forward, inward *and* outward, downward *and* upward may all be dimen-

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sions to be considered in the development of a psychoanalytic model of human existence.

1. *Super-ego and Identity.* In his contribution to this volume, Talcott Parsons relates his momentous integration of Freudian thought and modern sociology. He emphasizes the usefulness which the Freudian concept of the super-ego (as "the internalization of the social structures") has had for his attempts to link man's inner life and his social world. Let me, therefore, compare the super-ego as an earlier concept—both in the sense that the concept was created earlier in the history of psychoanalysis and in the sense that the super-ego originates earlier in life—with the identity concept.

The child internalizes into the super-ego most of all the prohibitions emanating from the social structure—prohibitions, furthermore, which are perceived and accepted with the limited cognitive means of early childhood and are preserved throughout life with a primitive sado-masochism inherent in man's inborn moralistic proclivities. (These are aggravated, of course, in cultures counting heavily on guilt as an inner governor.) Thus internalized infantile moralism becomes isolated from further experience, wherefore man is always ready to regress to and to fall back on a punitive attitude which not only helps him to re-repress his own drives but which also encourages him to treat others with a righteous and often ferocious contempt, quite out of tune with his more advanced insights. Man could not become or remain moral without some such moralistic tendency; yet without a further development of truly *ethical* strivings, that is, a subordination of his moralism to the shared affirmation of values, man could never build the social structures which define his adult privileges and obligations.

Such further development, however, is not taken care of by Freud's "structural" point of view, which is useful primarily in analyzing the extent to which a person has become a system to himself, unable to adapt to and to grow with the actual present. I have briefly accounted for the way in which a psychosocial theory can explain why adolescents must and can (as children cannot) join each other in cliques and "subcultures" and eventually join up with large-scale ideological trends of past or present, while a few close themselves up in malignant states of individual isolation equally unknown in earlier life. Here the strength of the ego seems to be dependent not only on the individual's preadolescent experience (including the contents of the super-ego), but also on the support it receives from adolescent subcultures and from the living historical process.

But in psychoanalysis, what has thus been learned about later stages of life must always be reapplied to previous observations on earlier stages

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and disturbances. It becomes obvious, then, that an intricate relation between inner (cognitive and emotional) development and a stimulating and encouraging environment exists from the beginning of life, so that no stage and no crisis could be formulated without a characterization of the mutual fit of the individual's capacity to relate to an ever expanding life-space of people and institutions, on the one hand, and, on the other, the readiness of these people and institutions to make him part of an ongoing cultural concern. All this, in fact, determines the nature of the identity crisis which, in turn, determines what happens to the remnants of the super-ego, even as later crises will decide the ultimate fate of the identity.

2. *Clinical and Actual Reality.* Psychoanalytic treatment will reveal much of the "scarcity economics" of an impoverished condition, in which much of the available energy has been directed inward as anxiety spread and anticipation became inhibited. But just because the "economic" point of view was invented to account for such simplified circumstances—simplified by the stereotypy of symptomatic behavior—they serve less well to understand the exchange of energies and, in fact, that shared multiplication of energies with a widening radius of individuals and institutions dictated by ongoing life throughout its stages. What is at stake here is not more and not less than psychological "reality." The clinical world view rightly takes regression and repression as its baselines and endeavors to fill in blind spots, to correct distortions, and to do away with illusions caused by infantile fixations. And, indeed, in removing all these, therapy creates a minimum condition for dealing with the world "as it is." But, in doing so, the clinician must take it for granted that his own function within the established order is "reality oriented."

In the absence of theories including such communal concerns, and in urban settings culturally predisposed to seek roots and sanction in intellectual explanation, some circles can come to cultivate a "reality" and a "normality" oriented toward a psychoanalytic utopia. Such considerations give due warning that a science so close to questions of health and ethics must include methods of observing its own functioning in the cultural-historical process and its (intended and unintentional) influence upon it. For mankind will always attempt to absorb insights into its unconscious as it absorbs other shocking truths, namely, with an adaptation combining overacceptance with deeper resistance. Always in danger of becoming part of the disease it meant to cure, a method bent on honesty must also remain vigilant enough to seek the cure for new disguises. The vigilance demanded here concerns the intrinsic dilemma of all research concerning man's motives: here explanatory assumptions are especially apt to become not only reductionist but also accusatory as well as

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excusatory, as if what is found to be true increased the guilt of some and absolved others.

The experience of the identity crisis, so I have indicated, takes place when the world of childhood gives way to that of an ideological universe which for a while coexists with the accumulative knowledge of "reality." True, youth gradually becomes equipped with all the cognitive functions which adult man will ever call his own. And yet, it is often radically involved in world-outlooks which make the "facing of reality" a hazardous criterion of creative imagination. To give this whole matter another dimension, I have insisted that the German *Wirklichkeit* really combines "reality" with "actuality," that is, a consensually validated world of facts with a *mutual activation* of likeminded people.[15] Even among the most intelligent and informed men, there is always a search for communality with those who not only think alike but also make each other feel active and masterly. And it must be obvious how often, in adolescence as well as in adult ideology, the most gifted minds must surrender their sense of verifiable fact to that of a mutual actualization through the sharing of a unified world image.

3. *The Inner and the Outer World.* The classical psychoanalytic technique was also the original research laboratory; how human nature works outside it was, by necessity, a matter of the speculative application of clinical findings to the "world outside." But psychoanalysis, as any other field, can renew itself only by gaining insight into the nature of its observational setting. The natural sciences have had to take account of the fact that not only the personal equation of old, but also all the details of the laboratory arrangements are apt to become inseparable from the bit of nature isolated for observation. In psychoanalysis, however, it is still claimed that only the clinical situation provides the setting that can reveal the true workings even of the "normal" mind. The authority often quoted in this context is Freud's statement that only a broken crystal reveals its structure which is invisible in intactness. But a living ego-organism is not a crystal, and even as anatomy and pathology must yield to physiology and biochemistry in the attempt to reconstruct intact functioning, psychoanalysis must complement its clinical findings with the study of psychosocial functioning.

It is, of course, true that no situation affords a better access to the workings of the unconscious than does the psychoanalytic one. Yet the greatest difficulty in the path of psychoanalysis as a general psychology probably consists in the remnants of its first conceptualization of the environment. To a patient under observation, the "world" he records (and more often than not, complains about) easily becomes a hostile environment—"outer" as far as his most idiosyncratic wishes are con-

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cerned, and "outside" his relation to his therapist. This says much about him and about man; but it seems difficult to account for the nature of the clinical laboratory if the nature of the human environment is not included in the theory which guides the therapeutic encounter. The fact is, such neglect sooner or later leads to the adoption of a clinical world view. When I once heard a revered teacher refer to the world at large as the "outer-psychoanalytic outerworld" I experienced a conceptual as well as a philosophical shudder. For even though it is true that, by definition, "the world" must always resist psychoanalytic insight, the acceptance and study of such resistance in its historical disguises is of the essence in the psychoanalytic enterprise.

A related problem is the application of what has been observed in the encounter of therapist and patient to other forms of relationships. In the psychoanalytic situation, early and earliest relations with significant persons is ever again transferred and relived, relost and renewed, and this (it seems probable) at least partially because of the technical choice of the basic couch arrangement by which the observer and the observed do not look each other in the face, communicate only with words, and thus avoid the earliest and the most lasting mutual affirmation: that by vision. Here, again, what is produced in the laboratory is highly instructive all around. But it presents a partially induced repetition of what in psychoanalytic theory is referred to as the search for an "object"—meaning both the original libidinal attachment to the mother as a whole person and the cognitive capacity to (literally) envisage her as a whole. This is truly basic; but it would seem wise to apply this "object" imagery only with caution to the functioning infant, as well as to any other person outside clinical captivity, and especially also to the young person whose needs during the identity crisis demand (beside some "parent substitutes") a vigorous peer group, an ideologically integrated universe, and the experience of a chosen mutuality with newly met persons and groups.

A close study of the original clinical setting as a laboratory would, incidentally, also reveal the fact that at least one vital aspect of life—namely, violence—systematically escaped the treatment situation, as if the individual would participate in any uses of direct violence (from aimless riot to armed force) only when absorbed in the unenlightened and uncivilized mass that makes up the "outerworld." But then, as pointed out, the clinical laboratory channels all impulse to act into introspection, so that the cured patient may be prepared for rational action. Thus, the clinician learns much more about the nature of inhibited and symptomatic action than about that of concerted action in actuality—with all its shared a-rationalities.

4. *The Ego and the I.* A final methodological problem may or may not point beyond psychoanalysis. Freud's puritanical self-denial as an

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observer in the long run prejudiced the very act of observing. Freud, it is true, went further than any man before him in revealing publicly the role which his own conflicts played not only in his own dreams, but also in his dealings with his patients. But in doing so, he failed to analyze the role of the observer who chooses to study conflict in himself as well as in others. He conceptualized the ego as a psychological part of man's unconscious inner structure, but he did not question the "I," the core of consciousness. But who wields this consciousness, so vastly refined as a weapon and tool in dealings with the unconscious? In using the word puritanical, I meant to imply that Freud's self-restraint in this regard, though it could be expressed in antireligious terms, was deeply religious in nature: man had talked enough of his soul, and had for too long congratulated himself on being "the measure," or the conscious "center," of the universe. If a man like Kierkegaard could write about the leap of faith, a doctor and scientist could only grimly and rationally describe with the ears and the methods given to him what many of the most sublime men as well as the most depraved had previously refused to acknowledge.

It is one thing, however, to cultivate the proud rationality of the Enlightenment of which Freud was probably the last great representative, and which he crowned by insisting that irrationality and the unconscious be included in the sphere to be understood rationally. It is another matter to derive from such inquiries into man's conflicts a model of man. At any rate, I have found myself studying in the lives of religious innovators that border area where neurotic and existential conflict meet and where the "I" struggles for unencumbered awareness. And again, is it not in adolescent experience that the "I" can first really perceive itself as separate? It does so, as it finds itself both involved and estranged in peculiar states which transcend the identity crisis in psychosocial terms, because they represent not only the fear of otherness and the anxiety of selfhood, but also the dread of individual existence bounded by death. All of this, of course, is easily forgotten when the young adult assumes his responsibilities and when he is forced to participate in the hierarchies of his society, with their organized beliefs and convincingly concealed irrationalities. But in the long run, the "I" transcends its overdefined ego and, sooner or later, faces the dilemma of existence versus politics which I have attempted to approach in my work on the Gandhian version of truth.

Behind all this may well be another and wider identity crisis. Psychoanalysis, in line with the Enlightenment, has debunked the belief (and the need to believe) in a deity. It has suggested that the god image "really" reflects the infantile image of the father, as, indeed, it does in transparent cultural variations. On the other hand, it may be for good phylogenetic reasons that the ontogenetic father is overendowed with

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an awe which can later be shared in common beliefs in god-images and in semidivine leaders. For a community of I's may well be able to believe in a common fund of grace only to the extent that all acknowledge a super-I. This is a problem of such magnitude that mere intellectual denial hardly touches it, least of all if this denial is accompanied by a displacement of divinity to "immortal" leaders. In the eyes of the young, the disenchantment practiced by generations of self-made men and of unbelievers has only sealed the demotion and abrogation of the fathers. The young, therefore, feel that there must be a concerted (if at times playfully anarchic) search for new bonds which will endow the modern person with a novel sense of communality and of actuality. At any rate, what religion calls grace and sin transcends the comfort of adaptation and the management of guilt.

All this takes us beyond the identity crisis in its developmental and psychosocial determinants. But, then, this crisis is (and makes sense as) only one of a series of life crises. What does happen, we may well ask in conclusion, to adults who have "found their identity" in the cultural consolidation of their day? Most adults, it is true, turn their backs on identity questions and attend to the inner cave of their familial, occupational, and civic concerns. But this cannot be taken as an assurance that they have either transcended or truly forgotten what they have once envisaged in the roamings of their youth. The question is: what have they done with it, and how ready are they to respond to the identity needs of the coming generations in the universal crisis of faith and power? In the end, it seems, psychoanalysis cannot claim to have exhausted its inquiry into man's unconscious without asking what may be the inner arrest peculiar to adulthood—not merely as a result of leftovers of infantile immaturity, but as a consequence of the adult condition as such. For it is only too obvious that, so far in man's total development, adulthood and maturity have rarely been synonymous. The study of the identity crises, therefore, inexorably points to conflicts and conditions due to those specializations of man which make him efficient at a given stage of economy and culture at the expense of the denial of major aspects of existence. Having begun as a clinical art-and-science, psychoanalysis cannot shirk the question of what, from the point of view of an undivided human race, is "wrong" with the "normality" reached by groups of men under the conditions of pseudo-speciation. Does it not include pervasive group retrogressions which cannot be subsumed under the categories of neurotic regressions, but rather represent a joint fixation on historical formulae dangerous to further adaptation?

But I have promised to pursue the matter of the "identity crisis" only as far as autobiographic considerations would carry it. At the same time I have recapitulated my conceptual ancestry insofar as it originates in

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psychoanalysis, in order to indicate that psychoanalysis represents a very special admixture of "laboratory" conditions, methodological climate, and personal and ideological involvement. Other fields may claim to be governed by radically different admixtures and certainly by much less subjective kinds of evidence. But I wonder whether they could insist, at any time, on a total absence of any one of the ingredients described here.

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EDWARD SHILS

Tradition, Ecology, and Institution in the History of Sociology

Sociology at present is a heterogeneous aggregate of topics, related to each other by more or less common techniques, by a community of key words and conceptions, by a widely held aggregate of major interpretative ideas and schemes. It is held together, too, by a more or less common tradition—a heterogeneous one in which certain currents stand out—linked to common monuments or classical figures and works. The tradition lives in a self-image which links those now calling themselves sociologists with a sequence of authors running back into the nineteenth century. Although in fact the main ideas which live on in contemporary sociology have a much older history, sociologists do not generally see themselves as having an ancestry originating any earlier than the nineteenth century.

Most of sociology is not scientific in the sense in which this term is used in English-speaking countries. It contains little of importance that is rigorously demonstrated by commonly accepted procedures dealing with relatively reproducible observations. Its theories are not ineluctably bound to its data. The standards of proof are not stringent.

There are differences, of course, among the various substantive fields of sociology, some being more scientific than others, but on the whole the standard of scientific accomplishment is low. This does not mean that in those parts which are not scientific there is not some very substantial learning or that there is not an accumulated wisdom which merits regard and consideration. Nor does it mean that some of it, even in its present intellectual state, cannot contribute to the improvement of policy and administration and to civility in a broad sense. It only means what it says, namely, that much of contemporary social science is not very scientific in the sense in which the term has come to be understood.

Nonetheless, sociology does exist. It has a history which present-day sociologists regard as their history—although of course the image of this history varies somewhat from country to country and among sociologists

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within particular countries. It has a vast and rapidly expanding stock of works and a large and also rapidly expanding personnel working in and forming academic institutions of research, teaching, training, and consultation; governmental institutions of research, execution, and consultation; and private, nongovernmental, nonacademic institutions in which sociologists perform some of the same activities they perform in governmental institutions. The stock of works, heterogeneous though it is in its particular subject-matters, in its techniques of observation and analysis, and in particular interpretations, is characterized by a few widely pervasive major ideas or beliefs about society, by a few major concepts or delineations of significant variables.

The concepts of social system, of society and its constituent institutions, of social stratification and social mobility, of elites and ruling classes, of social status, of role, of bureaucracy and corporate organization, of kinships and local community, of history and tradition, of intellectuals and ideology, of consensus, anomie, and alienation, of conformity and deviance, of charisma, of the sacred and rebellion against institutions—all these concepts have been grouped into a conception of modern society (and its variant of mass society) which is further defined by contrast with a conceptual construction variously designated as folk society, traditional society, or *Gemeinschaft*.

How has all this come about? Why has the intellectual stock of sociology come to be what it is and why has it taken that form in particular places? Why have certain ideas which are now thought to be constitutive of sociology come to dominate the subject?

One of the older answers was that social science or sociology could not emerge until men were capable of sufficient detachment from involvement in their own affairs and in their beliefs about these affairs; a corollary of this was that sociology could arise only when authority had lost some of its sanctity and when traditional beliefs became somewhat discredited. This view has been put forward by a number of writers, most notably, Durkheim and Sombart. There is something in this view—but not enough. The heavens belonged to the gods and religious beliefs and to those who took those beliefs in their charge on behalf of the gods, but that did not prevent the emergence of astronomy. It might have impeded the emergence of astronomy, but it did not prevent it. Man's body in the West was God's creation, but that, too, did not prevent the study of anatomy or the understanding of the circulation of the blood. In Greece, Rome, and medieval Islam, in the writings of Aristotle, Polybius, Thucydides, and Ibn Khaldun, ideas were put forth which have reappeared in modern sociology, but the subject never became precipitated in the way that physics or mathematics became established in the seventeenth and eighteenth centuries. Ancient Greece and Rome

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and medieval Islam were not secularized societies, they were not traditionless societies in which authority and custom had receded from earlier strength. So the hypothesis is not quite satisfactory. There is, however, something in the hypothesis that sociology requires a cultural matrix or setting which is not so exigent in its demands on substantive belief that it stifles detachment. There is something in the hypothesis that sociology requires a loosening of the belief that divine or magical powers intervene at will in human affairs. It is also true that sociology as a body of generalized knowledge about society requires freedom from a jealous and pervasive ecclesiastical or secular authority which is apprehensive about the potential dangers of the formation of beliefs not identical with those it holds about itself. These conditions, however, are the conditions of any intellectual activity which is not wholly committed in advance to agreement with the views held by ecclesiastical and earthly authority on matters which that authority believes are vital.

What I wish to say here is that the recession of earthly and ecclesiastical authority, and a loosening of the grip of traditional beliefs which consecrate that authority, is only a very general precondition. It tells very little about the intellectual direction and the territorial location of the growth of sociology.

Sociology, even if it is not very scientific, is an intellectual accomplishment. The practice of sociology—that is, sociological research and sociological reflection—is an intellectual activity. As an intellectual activity, it operates within an intellectual tradition and as such each sociological action takes place within the framework of the tradition which it in turn affects and, in some important instances, modifies markedly. The tradition of sociology even now is not rigorously coherent and authoritative in its presentation and it was less so in its earlier stages in the nineteenth and twentieth centuries. It has offered to its recipients a variety of possibilities. There has been a process of selection. The selection has been a function of intellectual self-evidentness—which is characteristic of any tradition—of exposure through individual contact, further fostered by institutional consolidation. Institutions have not created sociology; it has been created by individual sociologists exercising their powers of observation[1] and analysis on social situations apprehended within the focusing framework of sociological traditions. Institutions concentrate attention and reinforce selected ways of perceiving and interpreting experience. Institutions foster the production of works, and the works—with what they contain in the way of interpretation of social reality—become part of the focusing tradition. Institutions create a resonant and echoing intellectual environment. The sociological ideas which undergo institutionalization are thereby given a greater weight in the competition of interpretations of social reality.

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Institutionalization

By institutionalization of an intellectual activity I mean the relatively dense interaction of persons who perform that activity. The interaction has a structure; the more intense the interaction, the more its structure makes place for authority which makes decisions regarding assessment, admission, promotion, allocation. The high degree of institutionalization of an intellectual activity entails its teaching and investigation within a regulated, scheduled, and systematically administered organization. The organization regulates access through a scrutiny of qualification, provides for organized assessment of performance, and allocates facilities, opportunities, and rewards for performance—for example, study, teaching, investigation, publication, appointment, and so forth. It also entails the organized support of the activity from outside the particular institution and the reception or use of the results of the activity beyond the boundaries of the institution. An intellectual activity need not be equally institutionalized in all these respects. It should be remembered that an intellectual activity can be carried on fruitfully with only a rudimentary degree of institutionalization.

Sociology is more institutionalized where it can be studied as a major subject than where it can be studied only as an adjunct subject, where it has a specialized teaching staff of its own rather than teachers who do it only as a *Nebenfach*, where there are opportunities for the publication of sociological works in sociological journals rather than in journals devoted primarily to other subjects, where there is financial, administrative, and logistic provision for sociological investigation through established institutions rather than from the private resources of the investigator, where there are established and remunerated opportunities for the practice of sociology (teaching and research), and where there is a "demand" for the results of sociological research.

The social sciences today have become relatively highly institutionalized branches of study in the countries of Western Europe and of North America—more recently in the former than in the latter. They became established as academic subjects later than most of the other major academic disciplines, such as mathematics, physics, chemistry, zoology, botany, classics, oriental studies, modern languages, and literature. They have, however, now made up the distance. Degrees, undergraduate and postgraduate, are awarded for the completion of organized courses of study in them; research training under qualified teachers is provided, often as part of research teams organized and supervised by the teachers, reported preliminarily in seminars where work is criticized. Journals, with academically qualified referees and often supported by learned societies, are available for the publication of

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the results of research in each field and in some quite specialized subfields. In addition to more routine and periodic meetings of learned societies, there is an elaborate network of communication through the circulation of offprints, memoranda, and preliminary versions of research reports directed to both practitioners and apprentices in various fields and subfields, and even in the informally circumscribed domain of a particular problem. Practically all of these activities are firmly incorporated into the structure of universities and professional associations which have a momentum of their own.

In the first half of the nineteenth century, the social sciences scarcely existed as academic subjects anywhere, although of course they had forerunners in the *Staatswissenschaften,* in the juridical sciences, on the edge of historical-institutional studies, and in moral philosophy. Their academic establishment, which began slowly and unevenly in the last quarter of the nineteenth century, was made possible by a loosely articulated set of intellectual traditions which extend back to western antiquity and which acquired focus and delineation in the course of the nineteenth century.

The nodal points of these traditions were not generally university teachers, often they were not even university graduates. Indeed, not even in economics, which became an intellectually *orderly* discipline, relatively speaking, much earlier than sociology and political science, did the academic element have the field to itself. Adam Smith stands out as the first important academic contributor to the subject, although political economy was only one of the four subjects for which he was responsible within his jurisdiction as professor of, first, logic and, then, moral philosophy. (He had been out of academic life for thirteen years when *The Wealth of Nations* was published in 1776.) But Ricardo was never an academic, nor was James Mill, and the Reverend William Paley's teaching at Haileybury could not be regarded as strictly academic since that body was closer to a secondary school than a university. The major synthesist of the middle of the century, John Stuart Mill, had only the most marginal academic connections; he was rector of St. Andrews, a wholly honorific position and a very transient one. Nonetheless, Cairnes, Senior, Fawcett, Sidgwick, and Jevons were university teachers, and beginning not later than Alfred Marshall's first academic steps, serious young men could obtain at a few places in Great Britain an orderly and disciplined training in economic analysis. Empirical economic research was another matter, being usually in the hands of government officials, private amateurs, and voluntary bodies; it was largely avocational and minimally institutionalized. Even in this field, however, the possibility of acquiring guidance in the techniques of research existed within the academic frame. Thorold Rogers as profes-

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sor at Oxford and Archdeacon Cunningham at Cambridge and King's College, London, could help a young man to learn to do economic research with a quantitative bent. If we compare the situation with that prevailing in France, where economics was taught seriously only in the *Conservatoire des arts et métiers,* at the *École des ponts et chaussées,* and at other technical institutions where young men were being prepared for technological careers, and at the *Collège de France,* where there were no pupils, properly speaking, and came into the *Faculté de droit* in Paris only in the 1870's, where it was suffocated under legal lumber, we see one of the reasons why British economics led the world in the nineteenth and early twentieth centuries.

It must be emphasized that reference to institutionalization does not, by any means, wholly explain the great ascendancy of British economic analysis. The conception of the problems to be studied—which was partly a function of prevailing public opinion—had a good deal to do with it; the contacts of economists with the worldly affairs of Parliament, investigative commissions, governmental departments, and leading politicians and businessmen also gave them experiences in which they could exercise the analytical powers nourished by a rich tradition and a lively publicistic discussion. The British economists had a more fruitful point of departure because they analyzed the equilibrium of an economic system and not the economic necessities of governments. The latter task—partly arising from intellectual tradition, partly from the institutionalization of economics in administration—held back the development of economics in the German universities because it impeded the discovery of the market. This discovery and its subsequent elaboration were the great achievements of British economics between about 1775 and 1875 and in this achievement the institutionalization of economic analysis in the universities, governmental institutions, and the press played an important role. Although in this period academic institutionalization was rudimentary in contrast to what it later became, it was more advanced than in any other country. It was, moreover, reinforced—that is, attention focused and intellectual interchange intensified—by the high quality and density of public discussion in journals, books, investigative commissions, and parliamentary debates.

Sociology had several important figures who established the name of the subject—a significant step—and who studied its subject-matter. Auguste Comte and Herbert Spencer were the greatest of these forerunners. Neither of them was a member of the academic profession at any time in his career.[2] Tocqueville and Marx, who in substance are qualified to join the gallery of ancestors of present-day sociology, only recently have been nominated for retroactive membership in the tradition. They were neither more nor less academic than Comte or Spencer:

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they had no students, assistants, monograph series, or seminars; no dissertations were written under them.[3] Comte and Spencer installed themselves at the beginning of the tradition by giving a name and a vague set of boundaries to the subject still to be born.

Other originators—German historians and British and French statisticians—who in effect generated and impelled forward the tradition from which sociology as we know it emerged, have not been equally acknowledged. They are allowed to dwell in oblivion. But they are effective nonetheless.

1. The European Founders: Academic Cultivation Without Institutional Provision

The separateness of sociology from the universities during the first three quarters of the nineteenth century was reduced when founders replaced forerunners. The five European founders who stand between the forerunners and the academically established generations of the present century were all academic. Only one of them, however, was academically responsible for sociology. Ferdinand Tönnies was *Privatdozent* in philosophy and later, for several years, professor of economics and statistics; he taught sociology only after his retirement. Vilfredo Pareto never taught sociology; he was a professor of economics for about five years in his late forties. Max Weber, during his short academic career, was professor of economics. Georg Simmel taught philosophy as *Privatdozent* and *ausserordentlicher Professor* for most of his career and became *ordentlicher Professor,* but not of sociology, only a few years before his death. Emile Durkheim was the only one of the generation of founders who made a full academic career with an official responsibility for sociology. He was professor of sociology and education at Bordeaux from 1887 to 1902 and from then until his death in 1917, professor of the same subjects under various titles at the Sorbonne.

Of all the founders of sociology, only Durkheim was successful in institutionalizing his work during his lifetime. Only he had pupils and collaborators, regularly, over a long period. Sociology acquired an institutional form only around Durkheim and that was less through provision by the university system than through Durkheim's own organizational initiative and skill in the formation of the *Année Sociologique.*

Durkheim followed the German pattern: the professor of the subject covered the whole field himself, trained younger collaborators through intensive discussions, and published the results of their individual and joint work in the organ of the institute, which was reserved primarily for contributors from his personal circle. One difference was that

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in Germany the university provided funds for the institute, which was attached to the chair; in France there was no such provision for university professors. Another important difference between Germany and France was that in German universities those who worked with the professor in his institute were specializing in his discipline, were writing dissertations on it, and would depend on his sponsorship for *Habilitation* and subsequent academic appointment. There was provision for training in research at an advanced level in Germany from the early part of the century; in France training in research was provided only when the *École Pratique des Hautes Études* was founded in 1868. But neither in Germany nor in France did this affect sociologists in the beginning of the twentieth century. Sociology as part of the "human sciences" came into the EPHE only when the *sixième section* was formed after the Second World War. There were, furthermore, few opportunities under the French educational or administrative system for persons who had experience in sociological research. Those who "did" sociology did it because they were genuinely interested rather than because they expected to make a career out of it, because very few could. This meant that Durkheim's entourage consisted of intellectually deeply interested persons whose professional possibilities and aspirations lay outside sociology. While they were active they produced important work, but once they became active in their own careers, they on the whole produced very little which belonged to the core of sociology. It also meant that their reproductive capacity as sociologists was slight, since very few became teachers of sociologists. They did not train sociologists because there were very few students of sociology: there were very few students of sociology because there were practically no posts in sociology to which they could aspire.

In France, therefore, the institutionalization and the resultant expansion and continuity of production was generated almost entirely by the personal force of the individual professor. He was helped, of course, by the existence of the EPHE and the section on *sciences religieuses*, which was not dominated by professors.[4] Hence when Durkheim passed from the scene, the body of interpretation which had developed from his teaching and writing ceased to grow, and French sociology as the study of modern society practically disappeared for more than a quarter of a century. It is often said that the death in battle of some of the most distinguished of the younger collaborators of the *Année Sociologique* was the cause of the cessation of the Durkheim outlook in French sociology, and there is some truth in this. I think, however, that more important was the fact that the institutional structure built by Durkheim rested only on him and was not integrated into the institutional structure of the French university system.

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The fate of sociology in Germany supports this interpretation. The three great founders of sociology in Germany were not professors of the subject. They had no research facilities such as German professors ordinarily had; they had no ongoing seminar for training students in research and for bringing their dissertations to the point where they could be published as research monographs. There was no place for students to go professionally once they finished their training—had formally instituted training been available in Germany. Before the First World War there were no professorships of sociology in technical or teacher training colleges. There were no junior sociology posts in the universities, just as there were no senior posts. There was no employment for sociologists, as sociologists, outside universities. They had until the 1920's no journal of their own in which they could publish their works and thereby define their identity. In consequence, there was no concentration of minds on the ideas of great potentiality which came from the three German figures to whom the sociological world now looks back as founders. The ideas of Max Weber, Georg Simmel, and Ferdinand Tönnies did not undergo the process of reinterpretation, partial assimilation, and elaboration which is the characteristic course of development of intellectual products. The failure to do this reinforced the causes of the failure. At the University of Cologne there was, it is true, an active professor of sociology, Leopold von Wiese, who had students, a research institute, and a journal, those three important constituents of the institutionalization of an academic subject. Unfortunately for German sociology, von Wiese's ideas were incapable of development. He was concerned primarily with nomenclature and taxonomy and gave his students no other tasks. Institutionalization in this case could only have consolidated intellectual sterility.[5]

Brief attention might be devoted to another "founder," L. T. Hobhouse, professor of sociology at the London School of Economics from 1907 until 1929. Hobhouse had a widely ranging comparative interest. The breadth of his knowledge was comparable to Weber's and he organized one major piece of research which was unique, until recently, for its integration of quantitative methods into comparative study. Yet today Hobhouse is almost entirely disregarded by sociologists.[6]

Part of the explanation for the oblivion into which Hobhouse has fallen lies in the fact that his interpretation of modern society was felt to be an evolutionary optimism. In fact, it was in some respects not too different from Weber's or Durkheim's—coming as it did from traditions closely akin to or overlapping those which presided over the ideas of the continental founders of sociology. A large part of the explanation, however, seems to me to lie in the very low degree of institutionalization of sociology in British universities. Hobhouse was, with Edward Wester-

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marck,[7] the sole incumbent of a professional chair of sociology in the United Kingdom; his department was also the only department. Despite the sympathetic support of R. H. Tawney, there was no informal consensus among the more esteemed academic personalities regarding the value of the sociological approach to social matters. (Such a consensus began to appear only after the Second World War.) There were no undergraduates who read sociology for a degree in Hobhouse's time (it was not provided for in the London School of Economics syllabus), and British postgraduate students specializing in sociology must have been practically nonexistent. There were no academic appointments in sociology in Great Britain outside the London School of Economics.[8] There was one sociological periodical in England at the time—*The Sociological Review*—and it had no connection with the London School of Economics or any other academic institution. Its inspiration came from Patrick Geddes, Victor Branford, and ultimately LePlay, and its outlook was capable of intellectual development; it was, however, in the hands of a voluntary association of amateurs, enthusiasts, and cranks. There was no provision for research in sociology by students or teachers at the London School of Economics. The great tradition of empirical inquiry in Great Britain which had developed independently of the universities remained until well after the First World War and the work of the public-spirited men of means was done without the help of the universities.

Officially, the situation was not too different from that in France, but the strength of Durkheim's personality, the persuasiveness of his convictions and the superior prestige of the Sorbonne in the French university system—which the London School of Economics did not, at that time, possess—made a great difference. More important than the relatively low prestige of the London School of Economics during Hobhouse's career was the fact that the type of young collaborators available to Durkheim at the Sorbonne were not available to Hobhouse. Durkheim was able to draw to himself young scholars who were specialists in folklore, oriental studies, and ethnography, subjects which did not exist at the London School of Economics. Such studies were pursued in traditional ways at University College, London, and later at the School of Oriental and African Studies, where they had no contact with sociology. There were few postgraduate students and young lecturers at these other institutions and they had to make their careers by meeting the requirements of their own departments, which had a Germanic structure. Young Oxford and Cambridge dons in classics and oriental studies in the first quarter of the present century were unlikely to be drawn to a professor at the London School of Economics.

The fact that Hobhouse did not have Durkheim's imperious per-

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sonality and lacked his organizing enterprise, and the fact that evolutionism, even of the more cautious type cultivated by Hobhouse, enjoyed little intellectual esteem at the time in British academic circles, added to the obstacles to any influence Hobhouse might have exerted. Hobhouse's one distinguished protégé was Professor Morris Ginsberg, who succeeded him, but Ginsberg was a shy person and lacked Durkheim's organizing skills. The result was that the French sociology of Durkheim's circle left behind a massive deposit. British academic sociology—Hobhouse's variant of sociological evolutionism—left behind only the one work on the material culture of simpler peoples.[9]

The eugenics movement, which was not academically institutionalized, came in a roundabout way to have a more powerful impact on British sociology. It did so through its influence on the development of sophisticated statistical techniques by Galton, Pearson, and Fisher and through the short-lived Department of Social Biology at the London School of Economics in the 1930's. British studies in social selection and mobility belong to this tradition rather than to Hobhouse's.[10]

2. *American Founders: The Beginnings of Academic Institutionalization*

Sociology had a different fate in American universities. It became institutionalized earlier in the United States than anywhere else in the world; it became institutionalized earlier at the University of Chicago than elsewhere. Provision for teaching sociology at Columbia and Yale universities was made at about the same time as at Chicago, but by the turn of the century, provision for the training and research began at Chicago—hesitantly but encouragingly. There was not much to show at first. Research was occasional and discontinuous. In its first decade Chicago produced only minor and scattered pieces of research. The University of Pennsylvania (which sponsored W. E. B. Du Bois' *The Negro in Philadelphia*) and Columbia University did not lag markedly behind Chicago at this time. But, from the very start, there was a propitious atmosphere for research at Chicago, created, in part, by its youth, the first president's determination to make it into a research university (Gilman had already taken the lead in this at Johns Hopkins), and the large number of vigorous young professors who shared the ideal embodied in the research-centered seminars of the German university. Furthermore, because all the departments were new, there was no "old guard" to hamper the immediate establishment of a department of sociology.

Nonetheless, even at the University of Chicago, progress was slow. Albion Small was steeped in the German historical thought of his time,

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which he regarded as the matrix of sociology. He knew, however, that something more than historical scholarship was required and he was sympathetic to direct observational studies of contemporary events. He believed in seminars and postgraduate studies. Henderson, who was primarily concerned with urban welfare problems, also appreciated the desirability of descriptions and analyses of urban life, particularly the life of the poor, the immigrants, the working classes, and so forth. The Germanic sociological sensibility, intellectual curiosity, and a belief that what had been inherited was not enough all made for the conviction that a new type of research was needed. Nonetheless, even W. I. Thomas, who had an original mind and who received the third Ph.D. granted by the Department of Sociology, continued to work (at first) under the influence of German *Volkskunde,* ethnography, and physical anthropology on the basis of published sources.[11] Direct observational studies and the use of human documents emerged only gradually.

W. I. Thomas was, more than any other person, responsible for this new direction in research. He refined further a subject which had been developed in Germany[12]—the *Volkskunde* of living German rural society, based on impressionistic field observation and interviewing—but which had never found a place in German university studies in the social sciences. Thomas' readiness to observe directly, to collect the "human documents" of living persons, was supported by Small and Henderson. They did not think it undignified for a professor or the professor's pupils to wander about the streets and interest themselves in "low life." This was very different from the German academic situation; even those senior academic figures who were members of the *Verein für Sozialpolitik* and thus very concerned about the condition of the working classes thought that information about them had to be obtained through *Sachverständige,* that is, from middle-class persons who in a professional capacity—for example, magistrates, clergymen, municipal administrators, physicians, and so forth—were in contact with the lower classes.

This accomplishment of the Chicago Department of Sociology hardly seems to be a great intellectual accomplishment. Yet it marked the beginning of empirical sociology; it was the beginning of sociology as a potentially scientific, serious intellectual subject cultivated in a relatively institutionalized manner. This distinctive development went further when, at the beginning of the second decade of the century, Thomas succeeded in persuading Robert Park, who until then had led a somewhat errant existence as a newspaperman, a student in Germany, an assistant in philosophy at Harvard, and a secretary to Booker T. Washington at Tuskegee Institute, to join the Department of Sociology at Chicago. As a newspaperman interested in urban life, he was already acquainted with the surveys by which civically concerned bodies and

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individuals had attempted to arouse public opinion regarding the condition of the poor in the great cities. These surveys were the intellectual heirs of the American muckrakers of the turn of the century and of the British surveys of the preceding century. The surveys which attracted Park's attention had been conceived and carried out without benefit of academic sociology, and, when they were finished, the organizations which had been created to carry them out were disbanded.

The Chicago Department of Sociology changed this situation. Although the department itself never conducted surveys of the types conducted in Pittsburgh, Cleveland, and Springfield, it assimilated their techniques of direct observation, interviewing, and quantitative treatment—albeit elementary—of results into the training of postgraduate students.

Park brought to Chicago, in addition to this concern for direct observation and interviewing—one variant of which was the participant-observer technique—a fresh and vivid sense of the essential themes of German analytical sociology. Park had attended Simmel's lectures at Berlin, he read his works, and he absorbed certain of his views of modern society,[13] particularly those regarding the relations of urbanization to social differentiation and individuality.

This combination came to fruit just after the end of the First World War. Privately financed, nonacademic bodies concerned with race relations provided *debouchés* for sociologists (for example, the Carnegie studies on "Americanization" and the report on *The Negro in Chicago* requested by the Commission on Race Relations, supervised by Park and conducted by Charles Johnson). Civic and municipal organizations concerned with juvenile delinquency likewise provided support and employment opportunities as well as willingness to open their records to sociologists. Although Thomas was removed from the academic scene as punishment for a peccadillo, he and Park had already established a technique of research and a rough conceptual scheme which made sense of urban phenomena.

During the 1920's the pattern of institutionalization of sociology became well established. It centered on a standard textbook which promulgated the main principles of analysis, postgraduate courses, lectures, seminars, examinations, individual supervision of small pieces of field research to be submitted as course and seminar papers, and dissertations done under close supervision fitting into the scheme of analysis developed by Park, Thomas, and Burgess. It was sustained by the publication of the main dissertations in the Chicago Sociological Series and the transformation of the *American Journal of Sociology* into an organ of University of Chicago research. It was reinforced by public authorities and civic groups which offered sponsorship and cooperation

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for research, and by financial support from the university and private philanthropists. It was nurtured by a growing public interest in the subject, employment opportunities for sociologists as teachers in colleges and universities (mainly in the Midwest and Northwest), by a professional association (The American Sociological Society) which provided a larger forum and public for Chicago sociology, and by the summer institute of the Society for Social Research which maintained the solidarity of Chicago sociologists at home and away.

3. *An International Illustration of the Difference Made by Institutionalization: The Diverse Fates of Horkheimer and Mannheim*

The significance of institutionalization, even in the limited measure in which it was possible in sociology in Germany in the 1920's and the early 1930's, may be seen in the divergent destinies of the ideas of Karl Mannheim on the one side and of Max Horkheimer on the other. The difference in impact of the ideas of Mannheim and Horkheimer after they left Gemany also attests to the differences between Great Britain and the United States with respect to the institutionalization of sociology.

Mannheim was the more original and many-sided of the two: he had a more differentiated perception of contemporary society, a more vivid apprehension of particulars, and at least the same breadth of interest in macrosociological transformations. His knowledge of contemporary empirical research was greater than Horkheimer's and his ideas could have been translated into concrete research problems more easily than Horkheimer's. He wrote more and on more particular topics than Horkheimer. Mannheim raised important problems regarding the conditions of political detachment and partisanship among intellectuals, regarding the conditions of different forms of conflict and consensus among generations, regarding the influence of types of political partisanship on conceptions of historical time. All of these problems were related to matters of great contemporary interest, and they were capable of being empirically investigated. Yet Mannheim has had little influence, and Horkheimer is in a certain sense one of the most influential of modern social thinkers. Mannheim was, I have been told by his former students, a scintillating teacher, but in Germany he was a professor for only four years, and, although several very interesting dissertations were produced under his supervision,[14] the output of his pupils during his Frankfurt period was neither massive nor concentrated enough to provide a focus of attention and to create a far-reaching consensus as to what ought to be done and how to go about it. In 1933, Mannheim left Germany and

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went to the London School of Economics where, as in the time of Hobhouse and Westermarck, there were still very few postgraduate students of sociology, where there was no institutional provision for the organization, support, and supervision of research in sociology, where there was no organ of publication, and where there were no opportunities for the employment of those who had been trained in the subject. The Great Depression blocked numerous potential academic careers in Britain in well-established subjects, while the labor market for students of a fledgling subject like sociology remained at a standstill. The war years brought further attrition of what was already meager. Mannheim left the London School of Economics in the middle forties and became professor of the sociology of education at the Institute of Education in London. He died in the beginning of 1947.

The fortunes of Horkheimer's ideas were very different. The ideas themselves were relatively simple: modern society has become increasingly destructive of individuality as authority has become more concentrated and as organization has become more inclusive and more depersonalized; man has become a pawn for manipulation by others: the capacity for and the use of reason have declined. In many respects his ideas were like Mannheim's, although they were simpler and exhibited a slighter intimacy with the facts of contemporary societies. Horkheimer became in the course of several decades one of the most influential sociological writers of his time. He has certainly had a much greater impact on sociological work than Mannheim. Why was this so?

Horkheimer had the advantage of taking over the professorship of Carl Grünberg, a very productive teacher who had since before the First World War produced an admirable scholarly journal, the *Archiv für die Geschichte des Sozialismus und der Arbeiterbewegung*. In the middle 1920's Grünberg began to publish a series of *Beihefte*, which contained monographs on the subjects which came within the terms of reference of his chair—terms of reference which were becoming broader throughout the 1920's. As far as I know, the same wealthy patrons who supported Grünberg's work on the history of socialism and the labor movement took over the responsibilities for the activities associated with the chair when Horkheimer succeeded to it. The title of the chair was changed from "the history of socialism" to "social philosophy" and the *Archiv für die Geschichte des Sozialismus* was replaced by the *Zeitschrift für Sozialforschung*. The *Institut für Sozialforschung* had in fact existed before Horkheimer's accession to the professorship and to the direction of the new journal. Friedrich Pollock was the editor, but Horkheimer was obviously the leading spirit intellectually in the determination of editorial policy. The institute already had to its credit a number of large monographs, one of which

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was a major work of sinological scholarship by the then Marxist scholar, K. A. Wittfogel (*Wirtschaft und Gesellschaft Chinas*), and another, under commission, was a Marxist study of European thought by Franz Borkenau (*Der Übergang von feudalen zum bürgerlichen Weltbild*). The journal was changed in content from the history of socialism and the labor movement to macrosociology with a very strong Marxist and a less pronounced psychoanalytic bent. A large-scale collective investigation into authority and family structure—internationally and historically comparative, sociological, and psychoanalytic—was launched. It had many collaborators, each working alone on one problem. Another piece of research on "punishment and social structure" was historical and sociological in character.

This dense exfoliation would have been impossible without substantial financial support and the institutional apparatus made possible by that support. When Hitler's regime dismissed Jewish and socialist teachers, the Horkheimer institute went into exile. At first it went to France where it was given transient hospitality at the *Centre de documentation sociale* which Charles Bouglé had created and attached to the *École Normale Supérieure*. Since his patrons apparently had much of their wealth abroad, Horkheimer was able to emigrate to the United States with his most devoted collaborators, and to reassemble some of the others once he got there. Thanks, presumably, to Horkheimer's diplomatic and organizational skill, the institute established itself within the framework of Columbia University. Horkheimer was granted special status as a member of the faculty of Columbia University and he cultivated close ties with certain other members of the university. A collective institutional life was maintained and the journal continued, this time under the title *Studies in Philosophy and Social Science*. When the United States entered the war, three institute members found employment in the Office of Strategic Services. Meanwhile, other members were making their way as authors and as academic scholars. Near the end of the war, Horkheimer was engaged as research director by the American Jewish Committee, which granted him a sum of money—for that time very large—to conduct a widely ranging study of anti-Semitism. This afforded employment for a number of the stipendaries of the institute. The arrangement permitted the institute to adapt its viewpoint—an amalgam of Marxism, psychoanalysis, and patrician disdain for mass society—to American society and to fuse its point of view with the techniques of American social psychology and with the idiom then prevailing in American sociology. Franz Neumann and Otto Kircheimer became members of the government department of Columbia University, and exercised much influence over younger staff members and postgraduate students.

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The institute then returned to Germany, carrying with it what it had learned of American sociological and social psychological techniques and its own theory of mass society. It began at once a monograph series—not at all like the historical-sociological studies of the pre-Nazi period, but more based on field work, interviews, sample surveys, group discussions, and so forth. Horkheimer and Theodor Adorno came to rank with the leading intellectual figures of the German Federal Republic. Herbert Marcuse, after having played a minor role as a "Sovietologist" at the Columbia University Institute of Russian Studies, became a professor at Brandeis and then in California, and, by the cunning of history, if not of reason, became an intellectual idol of the New Left. After a career of warning against the dangers of freedom, Erich Fromm became the apostle of a society constituted by love and the sage of "socialist humanism." Leo Löwenthal became a professor at the University of California and an influential sociologist of mass culture, drawing on and applying in particular studies certain themes of the earlier writings of Horkheimer and Adorno. In Germany, the leading protégé of Horkheimer and Adorno became the main exponent of "critical sociology" and for a time one of the main intellectual inspirations of the *Sozialistsche Deutsche Studentenbund.* Karl Wittfogel joined the staff of the University of Washington in Seattle, and became a leading anti-Communist student of Chinese history, after having been a crude and aggressive Communist polemicist as a young man in Weimar Germany.

But the history of the *Institut für Sozialforschung* in Weimar Germany, the United States, and the Federal German Republic is not just the story of the cat that landed on its feet. It is a testimonial to the skill of a shrewd academic administrator, who by good luck and foresight inherited a favorable institutional situation and developed its connections within the various universities in which it was located, maintained its internal structure, *and* extended its connections outside the university. As a result it became the mechanism by which some of the most influential ideas of present-day social science developed. The doctrine of mass society, of the dehumanizing effects of the exercise of and the subordination to authority, of the role of the mass media, of the "power elite," owe a great deal to the *Institut für Sozialforschung.* It has not only provided the current ideas of the New Left, but it has also influenced and called forth a large amount of research among sociologists who had no direct connection with the institute. In contrast to this, Karl Mannheim, having created no following, has found none since his death, despite the repeated calls for a sociology of knowledge.

This digression about the *Institut für Sozialforschung* has been intended only to show the significance of institutionalization for the

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establishment of a subject. Institutionalization is not a guarantee of truthfulness: it only renders more probable the consolidation, elaboration, and diffusion of a set of ideas. It is not the sole determinant of the acceptance or diffusion of ideas. Intellectual persuasiveness, appropriateness to "interesting" problems, correspondence with certain prior dispositions and patterns of thought of the potential recipient are also very significant. Institutionalization serves however to make ideas more available to potential recipients, it renders possible concentration of effort on them, it fosters interaction about them, and it aids their communication. Insofar as it offers the possibility of a professional career in the cultivation of the particular intellectual activity, it both makes possible the continuity of exertion on a full-time basis and it adds a further motivation for its performance. The existence of practical or executive professions, which require the study of an intellectual discipline as a qualification and as a constituent of professional practice, provides a student body and teaching opportunities —and therewith research opportunities which develop in the interstices of teaching. In these ways, institutionalization makes a difference to the fate of ideas.

4. The Institution of Sociology and Academic Systems

The practice of sociology might be seen as the center of a series of concentric institutional circles. In Durkheim's case we saw the creation of an institution for sociological work, which did not attain, however, a full and formalized corporate structure in the production of the works themselves. Durkheim did not conduct seminars; he held informal discussions with his protegés. A corporate form was achieved in the organization and production of the *Année Sociologique* and the *Travaux de l'Année Sociologique.* The circle which Durkheim formed was a rudimentary institutionalization. It had various ad hoc connections with other institutions, for example, with the *Ecole Pratique des Hautes Etudes* and the *École des Langues Orientales,* where the informally adherent members of the circle had their employment, and with Felix Alcan, the publisher of their works. Durkheim himself held a professorship at the Sorbonne and he therefore was an incorporated member of an academic institution: it was not, however, in that capacity that he organized the work of his circle. He created a proto-institution with only peripheral and fragmentary institutional connections.

Max Weber's activity as a sociologist was much less institutionalized. He did not teach sociology; he supervised for a limited period a set of research projects for the *Verein für Sozialpolitik;* he tried to institu-

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tionalize two research projects on the press and on voluntary associations through the *Deutsche Gesellschaft für Soziologie* (he failed); he wrote *Wirtschaft und Gesellschaft* as one section of a comprehensive series of handbooks on economics, organized by the publisher Siebeck; he edited a great journal of social science and social policy, very little of which was devoted to sociology. The connection of his sociological activities with institutions was peripheral, fragmentary, and transient.

Thomas and Park were more institutionalized in their practice of sociology: they taught the subject regularly within the framework of a systematic course of study, organized toward the granting of university degrees; they supervised the research of students who were working toward postgraduate degrees. They themselves conducted organized research projects, employing assistants or collaborators, supported by university or externally granted funds. Many of the students whom they trained went on to sociological careers as teachers and research workers. Thomas and Park occupied a constitutionally provided position in the structure of the university. They were linked relatively densely with civic, governmental, and private bodies which were interested in the results of their research and which encouraged them by their interest and occasionally by their financial support as well as by accommodating research workers in their midst or under their auspices. In the 1920's private philanthropic foundations established grants for sociology and the Social Science Research Council established predoctoral and, later, postdoctoral fellowships for the promotion of sociology, some of which were held at the University of Chicago. During the Great Depression, Chicago sociologists found employment as social statisticians in a number of government departments and agencies. Thus Chicago sociology, that is, the sociology of Thomas and Park, was institutional at its center and in a fairly dense network of connected institutions removed at various degrees from the primary educational and research processes.

The primary institutional system of sociology is thus affected by its linkages with the environing institutional context: the university itself, foundations, civic bodies, government, business firms, publishing enterprises. The availability of the latter is of some consequence for the internal, primary institutionalization of sociology, in the provision of legitimacy through the sponsorship of established institutions, resources, employment opportunities, and so forth.

The study of the establishment and diffusion of sociology and the influence of this process of institutionalization on the substantive composition of the sociological traditions cannot be confined to the study of primary institutionalization. It is necessary to ask why sociology was able to become institutionalized at a particular time in the United

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States when it had not become equally institutionalized in Europe, although at the same time the intellectual accomplishment of European sociology was greater than that of American sociology.

To account for this we must go beyond the tradition of sociology and beyond the primary institutionalization of sociology. We must consider the social structures which permitted it or inhibited it or fostered it. The academic systems first of all. In Germany the creation of a new chair depended on the consensual decision of other professors in the same faculty—rivals—for resources and prestige and the approval of the university senate and the state minister of education. A new subject might be created by a *Privatdozent,* but it could not by virtue of that become established in the university. An old university had an established allocation of resources, and its beneficiaries would not readily allow it to be changed in favor of a subject lacking the legitimacy of age and accomplishment. New universities were more likely to allow new subjects; thus sociology was given the dignity of a professorial chair first in Cologne, founded (for the second time) in 1919, and in the University of Frankfurt, founded as a private university in 1914. It became established as a teaching subject more easily in technical colleges than in universities. It was more sympathetically viewed by ministers of culture after the republican regime was established at the end of the First World War.

In France, as in Germany, universities were immobile. The oligarchy of established professors of a faculty and the high degree of centralization of control over the total university system in the hands of the national ministry of education hindered the creation of new chairs for new subjects. In British universities, although a chair was created at the London School of Economics in 1902 (on a private endowment), no additional chair was created until after the Second World War.[15] At Oxford and Cambridge, the matter was not even canvassed until after the Second World War and there the democratic oligarchy of the representatives of established subjects prevented the diversion of resources to a subject of questioned legitimacy.

In the United States, in contrast to the European academic system, the universities were independent of central control and professors of established subjects did not rule the universities. Sociology first became institutionalized in the era of the autocratic university president. Such a president could create a new department if he could persuade the board of trustees to agree and could raise the financial resources to pay for it. The availability of private financial support and the practice of its active solicitation gave a flexibility to university budgets which the European universities did not have.

It was not, however, only the structure of government of the universi-

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ties which facilitated the earlier establishment in the United States. There were in the midwestern American culture of the period of the establishment of sociology a number of features which made a large difference. (It was in the Midwest that sociology first became academically established—in Chicago, Wisconsin, Michigan, primarily, but also in Indiana, Iowa, Nebraska, and Illinois.) The intellectual leadership of the Midwest was antagonistic toward the hegemony of the older eastern universities; they were distrustful of what they thought was the excessive respect of those universities for the past. They thought that knowledge was not degraded by being about contemporary things. There was, in short, no hard, thick incrustation of genteel, traditional, humanistic, Christian, patrician culture such as prevailed in the eastern universities. The hierarchy of deference was weaker in the Midwest; there was more equalitarianism, greater sympathy for the common life, more understanding for ordinary people, and, therefore, more readiness to be intellectually concerned about them.

Among midwestern intellectuals—publicistic and academic social science intellectuals—there was a more critical attitude toward the activities of the business class—industrialists, railway magnates, and bankers of the Eastern seaboard—and a greater skepticism about the adequacy of the classical economic theory which was adduced to explain their actions. It was in the Midwest that "institutional economics," developed most notably by John R. Commons, emphasized that there was more to society than what was accounted for by classical economic theory.[16] Sociological jurisprudence was part of the same atmosphere; in the United States it was largely the work of Roscoe Pound from Nebraska and Brandeis, an outsider by origin to the dominant culture of the educated classes of the Northeast.

Quite apart from the cultural relations of the Midwest and the Eastern seaboard, it should be pointed out that the midwestern universities, even where they were not established after the Civil War, became intellectually expansive only after that war. Harvard, Princeton, Columbia, Yale, Pennsylvania had developed strong cultures of their own before the Civil War and they were, therefore, less receptive to the German academic culture which was being brought back by an increasing number of young men with scientific and scholarly ambitions in the post-Civil War period. German historicism, which was one of the main sources of sociology, and the conception of the university as a scene of teaching *and* research were more readily received in the Midwest—at Chicago, Wisconsin, and Michigan especially—than in the older universities of the East. It might, indeed, be said that the culture of the German universities from which Max Weber's sociology grew but which could not produce an academically institutionalized sociology in

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Germany because of the structure of German university was able, upon transfer into an academic structure of greater flexibility, to realize its sociological potentiality.

5. Sociology and the Larger Cultural Tradition

In Great Britain empirical sociology grew up in the setting of a cognitively oriented tradition of the discussion of the exercise of authority. From Bacon to Bentham, a tradition of belief developed in Great Britain which asserted that systematically gathered empirical knowledge could be an instrument for the improvement of man's estate. The great civil servants who encouraged the system of social reporting to accompany the legislation of Benthamite inspiration were themselves the heirs of this tradition. Field surveys and the acquisition of information from experts and experienced persons through questionnaires and depositions before commissions of enquiry—in these ways knowledge was to be acquired and assimilated into the process of passing laws and verifying their efficacy. The ancient English universities and the Scottish universities did not share this attitude toward knowledge, and public opinion outside the universities did not share it sufficiently to pervade the universities and cause them to change their minds. It was confined to limited circles of politicians, businessmen, and administrators.

The situation was otherwise in the United States. Not only was there a fairly widespread belief in the value of being in possession of *les lumières*, of the better *quality* of a life illuminated by knowledge of nature and of man, but there was also a widespread belief in the value of knowledge as an integral part of the action of betterment. The federal establishment of the land grant colleges and generous support by midwestern state legislatures were inspired by this belief. In Wisconsin, academic social scientists, perhaps earlier than anywhere else, were summoned by politicians to aid them in the drafting and execution of legislation. Robert Park probably never read Bentham but he believed that social surveys were an important part of reform movements, and in his analysis of "collective behavior" he used to point to the role of the "survey movement" as a stage in the mobilization of belief about what had to be done to improve conditions. This cognitive orientation toward the organization of political action, coupled with an untraditional belief in the legitimacy of knowledge of contemporary things, contributed to the willingness of wealthy individuals and organizations to support sociological research. It gave sociological research a ground for self-respect at a time when its intellectual achievements were insufficient for that purpose.

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This is not to say that the substantive content of the sociology which came to be established with the support of this extra-academic attitude was determined by this attitude. It does mean, however, that the prevailing extra-academic culture contributed to the institutionalization of sociology in the United States. It also means that the subject matter of sociology and, to some extent, its techniques—in the United States as in England—were selected by criteria which derived from this extra-academic culture. The principles of interpretation—the substance of the central tradition—derived, however, from more purely intellectual (and intra-academic) sources.

Ecology: International Movement

The development of an intellectual field or discipline may be viewed as including an ecological process as well as a process of institutionalization. Intellectual constructions have a spatial aspect. Their production and cultivation take place in space; institutionalization takes place at particular points and it expands or contracts territorially as well as varying in the direction of less or more institutionalization. Institutionalization is one of the factors which affects the direction of spatial movement of ideas, just as it is a mechanism of the elaboration, promotion, or suffocation of ideas.

(1) *From Germany:* Ideas are likely to move with more momentum if they are institutionalized at their place of origin—just as they are more likely to be taken up, elaborated, and "used" (at least up to a certain point) the more institutionalized they are at their place of reception.[17]

Max Weber was located in Heidelberg from 1896 to 1918. It was in Heidelberg that he developed his most significant sociological ideas. He did not teach during most of this time, and his influence was confined to personal friends and his "circle." There was no established academic except Ernst Troeltsch, who worked out any of Max Weber's ideas. Ernst Troeltsch was professor in Heidelberg from 1894 to 1914 and while there he published his *Die Bedeutung des Protestantismus für die Entstehung der modernen Welt* and *Die Soziallehren der christlichen Kirchen und Secten*, the crucial ideas of which Max Weber in his turn elaborated, amplified, and set afloat in the world.

Before the First World War, Weber seems to have been quite unknown in the rest of Europe and in the United States, despite his journey to the St. Louis Exposition and his publication of one paper in English in the proceedings of the conference held there. Troeltsch's *Bedeutung* was translated into English in the Crown Theological Library and this too passed unnoticed by English-speaking sociologists.

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In France, Weber seems to have caused not even a ripple. Durkheim reviewed Marianne Weber's book on the legal status of women—rather slightingly—but he did not discuss Max Weber. I do not know of any reference to Weber in France aside from those contained in the French translation of Sombart's *Der Bourgeois*. The two leading Germanists in France at this time were Charles Andler and Lucien Herr. Neither of them seems to have paid any attention to Weber. In Italy, too, Weber was unknown.[18] Pareto never referred to him, nor did Mondolfo or Antonio Labriola. Only his protégé, Roberto Michels, who in 1907 had become a *Privatdozent (libero docento)* in Turin referred to his writings, but there was no echo.

Simmel was better known. In France the *Année Sociologique* published a translation of his monograph *Über Soziale Differenzierung*. Durkheim's early approval changed markedly: both *Soziologie* and *Die Philosophie des Geldes* were censured by Durkheim, and that was the end of Simmel as far as French sociology was concerned. In the United States various essays of Simmel appeared in English translation in the *American Journal of Sociology*, and this before the appearance in Chicago of Robert Park. It was undoubtedly Small who was responsible, but there is no trace of Simmel's ideas in Small's work. Only when Robert Park came to Chicago did Simmel's ideas take up residence in Chicago sociological research. Simmel's influence received its definitive expression in Wirth's "Urbanism as a Way of Life," whence it became a common possession of American sociology.

Tönnies, despite his sojourn in Great Britain, his major contribution to Hobbes's studies, his close affinity to Henry Sumner Maine, and his great emphasis on quantitative work, found no response in British sociology. Durkheim knew *Gemeinschaft und Gesellschaft* and, despite a certain community of outlook, criticized it sharply; his adherents made no use of Tönnies' ideas. In the United States, on the other hand, his ideas were resonant in Park's writing and teaching. Like Simmel, his main theorems entered into American sociology.

Weber had close relations with Heinrich Rickert and Karl Jaspers in Heidelberg but they were in philosophy and psychiatry and had little to do with sociology. There was no professorship in sociology in Heidelberg and the *Archiv für Sozialwissenschaft und Sozialpolitik* covered all the social sciences, devoting relatively little space to sociological writings. Weber shared the editorship of the *Archiv* with other scholars whose interests were less definitely sociological. His intellectual-convivial life was spent in a diverse company which included philosophers, jurists, and literary critics, and Weber neither gave it a focus nor shaped its output in the manner in which Durkheim had done with his entourage.

In the next generation, Karl Mannheim was Weber's main sociologi-

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cal continuator. Mannheim was in Heidelberg through the 1920's. His main personal connection was with Alfred Weber; from him he adapted the idea of *freischwebende Intelligenz.* He attended Marianne Weber's colloquia. Max Weber's ideas were a living presence, but only a very general one at Heidelberg at that time. None of the numerous professors and *Privatdozenten* who professed sympathy and admiration for Weber's ideas undertook to expound them systematically or to apply them. Mannheim was the exception.[19]

It was Weber's ideas on bureaucracy which Mannheim absorbed and which he tried to elaborate in his essay on *Erfolgsstreben* and which later became the central idea in *Mensch und Gesellschaft im Zeitalter des Umbaus,* which he wrote and published while in exile. It was also an idea of Weber, the idea of the charismatic revolutionary directly inspired by a believed contact with ultimate things, which Mannheim adapted in the essay on the utopian mentality (*Ideologie et Utopie*) and which he took up again toward the end of his life in his development of the notion of "paradigmatic experiences" (*The Diagnosis of Our Time*).

Mannheim's relations with Weber's ideas were affected by the fact that he came to Heidelberg only after Weber had died, that his mind was confused by an appreciation of Lukacs' type of Marxism and his awareness of its untenability, and that he worked in Heidelberg in a situation in which there was no systematic analysis or discriminating application of Weber's substantative sociological ideas.[20]

When Mannheim went to Frankfurt he replaced Franz Oppenheimer, who had been a *Privatdozent* and finally a professor after nearly twenty years in private medical practice and whose sociological ideas, written down in an immense eight-volume treatise, had no intellectual after-life at all. Mannheim brought Weber's ideas to Frankfurt.

The image of mass society was developed in the 1930's by Mannheim, Horkheimer, and Lederer. One source was the patrician tradition in Germany of the German *Bildungsschicht* and its contempt for bourgeois culture; another was the experience of the National Socialist movement in the last years of the Weimar Republic. But much of it came from an interpretation of Weber's ideas of bureaucracy and the charismatic type of authority, through Mannheim's *Mensch und Gesellschaft im Zeitalter des Umbaus,* which was one of the very few German works to bear the impress of Weber's ideas. Lederer's treatment of mass society also had its intellectual roots in the Weberian atmosphere of Heidelberg where Lederer had been a *Privatdozent* before going to Berlin.

For the rest there was scarcely any impact of Weber's ideas on German sociology. Some of the work on white-collar employees owed

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something to the stimulus of Weber's writing on bureaucracy, although I think that only Hans Speier's work is clear-cut in this respect. (Speier was a pupil of Mannheim.) The professors of sociology in Germany in the Weimar period—Vierkandt, Rumpf, von Wiese, Geiger, Freyer, Meusel, Dunckmann et al.—were proper German professors in the sense that they were rivalrous toward their peers. Most of them wrote books of "principles" which were largely variations on the theme of *Gemeinschaft* and *Gesellschaft*. They made little use of Weber's work. They were, however, unlike the classical type of German professor in that they created nothing important of their own and they trained no *Nachwuchs*. Albert Salomon, who was a professor in a teacher-training college, was a devotee of Weber but he had no products and no heirs. Paul Honigsheim was another in a position which had no influence.

Thus Max Weber faded out of German sociology in a decade when a number of professorships in sociology were created in universities and technical colleges. It was an outsider to Heidelberg and Germany who did more for the animation of Weber's sociological ideas than anyone else. Talcott Parsons came to Heidelberg in the middle of the 1920's. Having studied "institutional economics" under Walter Hamilton at Amherst and being endowed with greater analytical powers than his older contemporaries, he was quick to see that Sombart gave a more sophisticated form to the antitheoretical attitude contained in American "institutionalism" and that Weber succeeded in doing justice to what was valid in the intentions of institutionalism while avoiding its errors. Parsons' Heidelberg dissertation on capitalism in the writings of Sombart and Weber was the first in a long series of publications which sustained and extended the intellectual accomplishment of Max Weber.

On his return to the United States, Parsons became a tutor in economics at Harvard; there was no sociology there at that time. The institutionalized obligations of the tutorial role forced him to study economics proper in a department which was already very eminent, thanks largely to the presence of Joseph Schumpeter and F. W. Taussig. The study of Marshallian economics led Parsons to put Weber and Marshall into a common set of categories and thus enabled him to see what was distinctive in each. Pareto was pressed upon his attention by Lawrence J. Henderson, the distinguished physiologist who was a powerful figure at Harvard, a man of wide-ranging curiosity and a great deal of intellectual self-confidence. Henderson had been giving a seminar on Pareto for some time. Parsons' preoccupation with Durkheim was reinforced by the interest which Elton Mayo, T. N. Whitehead, and the Western Electric Research Group at the Harvard Business School had in *anomie* as one of the "human problems of an industrial civilization."[21]

By no means all contacts with an intellectual tradition are instigated

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through personal contacts provided by membership in a large, differentiated institution. Traditions can be sought out and found with only a minimal degree of institutionalization. Parsons' bringing in Durkheim and Pareto heightened contrasts, disclosed uniqueness more sharply, and revealed common elements.

What emerged from this synthesis were a number of themes which came to be taken up in various ways in the quarter of a century which followed the publication of *The Structure of Social Action.* One of these themes was the assertion of the importance of value-consensus as a major variable alongside individual and corporate ends (including definitions of "interest"). This in turn was connected with another theme regarding the importance of beliefs about the legitimacy of authority in society as regulators of perpetually present potentialities of conflict between parties of conflicting interests. Another set of themes, centering about categories for the description of action, was promoted through Parsons' study of the medical profession; this was further developed into an analysis of the professions in modern society, which, a few years after the publication of *The Structure of Social Action,* specified some of the properties of the moral ethos of the learned professions.

To do this, Parsons drew on Tönnies' conception of *Gemeinschaft,* which most writers had interpreted as a total antithesis of *Gesellschaft* (which was equated with modern capitalist society). The Tönnies dichotomy had been assimilated but never systematically analyzed into its component elements by Weber. Parsons went further and began the process of an analytical breakdown of the conceptions of *Gemeinschaft* and *Gesellschaft* into their components. This was subsequently "completed" in *Towards a General Theory of Action*[22] where the pattern-variables were systematically elaborated as affectivity-affective neutrality; diffuseness-specificity; particularism-universalism; collectivity orientation-ego orientation; and ascription-achievement orientation. All of this development was an extension of Weber's substantative ideas.

(2) *From France:* The sociological ideas of Durkheim largely died away in France not very long after Durkheim himself died, as far as their application to modern society was concerned. Davy, who published an important Durkheimian treatise in the early 1920's, entered educational administration and became a professor of sociology only a quarter of a century later. (Had there been some sociology in the universities in the 1920's and 1930's, Davy as a senior official would have been in a crucial position to promote its institutionalization. Unfortunately, there was practically nothing for him to promote.) Bouglé, too, became an administrator and ceased to do sociology. Simiand worked as a government

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official and only later in his life became a professor—of labor history. Granet continued to be a faithful and creative Durkheimian and a great sociologist, but his teaching was entirely sinological and he had no students of sociology. Mauss became professor of the "religion of the *non-civilisées*" early on and later became director of the *Institut d'Ethnologie* at the Sorbonne as well as professor at the Collège de France; in none of these capacities did he have students of sociology understood either as a general theory of society or as the study of modern societies. Durkheim left behind an extraordinary band of disciples in specialized fields which were at the periphery of sociology, but since sociology was not institutionalized at the center, their accomplishments remained dispersed. There was no consolidation and no sociological *Nachwuchs*.

Only Halbwachs continued the Durkheimian tradition as a sociologist studying modern societies, but aside from his own work, there was little extension. Why was Halbwachs unable to reestablish a school extending and deepening the traditions flowing from Durkheim? Halbwachs had a vast range of knowledge—perhaps wider than Durkheim's —great working capacity, and high technical skill, and his was a more supple and sinuous mind. But nothing came of his work in the sense of its being taken up by younger French sociologists and entering into the course of sociological development. (Of course his own books still stand as monuments.)

The answer does not seem too difficult. Halbwachs was a professor at Strasbourg, a distinguished university, but not one to which the best students flocked from all over France. At Strasbourg, Halbwachs as a sociologist was alone. He did not, as far as I know, supervise doctoral dissertations, this privilege being reserved for Parisian professors. He had no younger colleagues to associate themselves with him. Thus, this powerfully and sensitively intelligent man who carried forward the Durkheimian tradition, but in no uncritically submissive way, had no juniors of sufficient quality and in sufficient numbers on whom to leave a mark. The intellectual society around him was not dense enough; the necessary complement of institutions—specializing students, postgraduate training, research projects and assistants, and journals—were not available to him.

Durkheim's influence could, of course, have survived in a free-floating form. Not all intellectual filiations need be transmitted by an apostolic succession within the framework of a corporate body. Influences need not inevitably be institutionally transmitted and impressed by reenactment in research and publication under supervision and in collaboration. Durkheim's tradition failed, however, to operate in the way in which literary traditions operate—works set loose in the world to list whither they will, finding reception here or there at the

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discretion of or in accordance with the disposition of the recipient but without the reinforcement by the discipline of systematic and supervised study under an authority and with the support of peers whose critical acceptance facilitates ones own critical acceptance.[23]

For founding ideas to travel across national boundaries in subjects which have no universally apprehensible symbolism, language barriers must be absent or there must be many translations. There must also be, as in the case of the Weber–Parsons relationship, a strong intellectual personality who had immersed himself deeply in the founding ideas and who can sustain the newly acquired ideas either with the aid of institutions in the new setting or through sheer force of intellectual character.

Durkheim's ideas traveled little because there was no one to carry them. Ziya Gökalp took them to Turkey but that was a dead end because Gökalp, although he taught sociology at Salonika and became professor of the subject in Istanbul, was not a strong intellectual personality; he was more interested in legitimating Turkish nationalism and modernization than in sociological research. Nor did he have research students. If he had any, there would have been no careers for them. There was nothing in British sociology of the period before the Second World War which showed Durkheim's influence. Durkheim visited England once, I think, at the beginning of the century and delivered a lecture before the Sociological Society justifying the existence of sociology. The brevity of the visit, the inconsequentiality of the subject, and the absence of receiving institutions formed no bridge over which Durkheim's ideas could travel. Sociology did not exist as a university subject in England at the time. Sociology was a subject of the laity which was interested in eugenics, evolution, and the condition of the poor. There was nothing in Durkheim which made an immediate sympathetic connection with any of these.[24]

Germans took no interest in Durkheim's ideas, which were available to the German public through the surveys of sociological theories by Squillace and Sorokin and in a dissertation of a Roumanian named Georges Marica. Since German sociologists paid so little attention to Weber, it was scarcely likely that they would clasp Durkheim to their bosoms. Germany was really out of the question as a place for Durkheim to be influential. Before the First World War Germany was the academic center of the world. German scholars of that period could, according to their fuliginous lights, be courtly, but they really did not regard foreign science or scholarship—except perhaps in Islamic studies or mathematics—as worthy of serious consideration. And sociology?! German scholars were not acknowledged academically in that field—although Weber, Simmel, Troeltsch, Tönnies, and Sombart were men

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of very great merit and Germans to boot—so why take a Frenchman seriously? All this in addition to the deteriorating political relations between France and Germany at that time.

(3) *From the United States:* In the United States before 1914, sociology was either under the dominion of German-inspired ethnographers and *Volkskündler*—for example, W. G. Sumner—or it was conscientiously making its first contacts—unprecedented except for Charles Booth, Seebohm Rowntree, the forgotten French surveys of *les classes dangereuses*, and the British poor law and sanitary inspectors—with the urban masses. It had no use for theory. (The American sociologists were—and still remain—ignorant of Halbwach's prewar researches on the development of needs in the working classes.) There were only two American sociologists capable of receiving the Durkheimian influence before the First World War and to whom it would have made a difference had they experienced it. One was William I. Thomas whose study of the European sociological literature ceased as he immersed himself in his great inquiry into the Polish immigrant in America.[25] As far as I know, Thomas was quite ignorant of Durkheim; he certainly did not regard him as a source of his ideas, although as a matter of fact his views in many matters were very close to Durkheim's.[26]

The other was Robert Park, who had studied in Strassburg, Heidelberg, and Berlin with Windelband and Simmel, but whose knowledge of Durkheim—and Weber for that matter—was slight or indifferent.[27]

A Columbia Ph.D. named Gehlke, later professor at Ohio State University, wrote a dissertation on Durkheim, mainly dealing with the definition of a social fact, but scarcely touching substantive matters. Durkheim had been given a bad name by Alexander Goldenweiser in 1916 and this impeded his acceptance in the United States.

It was, in fact, Professor Parsons who became the chief protagonist of Durkheim in the United States. He was reinforced by Elton Mayo.[28]

Robert Merton, who first studied Durkheim under Professor Parsons and Sorokin, has perhaps given the greatest impetus to the diffusion of one element of Durkheim's analysis of modern society, namely, to the analysis of *anomie*. His elucidation of the components of the phenomenon of *anomie* has been taken up by a number of investigators—one of them his Harvard contemporary, Leo Srole. It has now become, like charisma, part of the stock in trade of American sociology.

(4) *From Great Britain:* The relatively uninstitutionalized structure of British sociology up to 1914 permitted a great miscellany of sociological tendencies to have a stunted and erratic existence. Some of them, such as the eugenics current, came subsequently to have a great impact on American and then on world sociology, through the development

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of refined statistical measures and a less weighty but still considerable influence on the study of social mobility. (Francis Galton was in both cases the great progenitor.) They did not, however, come to America at once. Much more important was the social survey method in the study of the urban working classes. The Booth and Rowntree surveys had a marked influence on American surveys, although American academic sociology did not go in for sample surveys until after the First World War. The Middletown survey was the first major American survey and the technique received a tremendous impetus from the development of public opinion polls from the 1930's onward. Robert Park assimilated their lessons and adapted them to the financial position of Ph.D. research at the University of Chicago.

The evolutionary sociology deriving in part from Spencer did not find a response in the United States, except in the work of W. G. Sumner and the early works of W. I. Thomas. Family studies, which were mainly study of family disorganization, were not influenced by Westermarck. They derived rather from the British social survey tradition, with its interest in the conditions of life of the poor. They were also to some extent influenced by the German standard of living studies which had been initiated by Engel.

(5) *A Note on Center and Periphery:* At the end of the nineteenth century, the three leading countries of the world were Great Britain, France, and Germany. In the academic sector of intellectual life, Germany was preeminent. Sociology, being academically the least institutionalized of the scientific disciplines, was not in a position to offer *actively* much to any other country. Internationally, its traditions were freely floating and it is not surprising, therefore, that each of these countries acted as if the others scarcely existed sociologically.[29]

In contrast, the United States went to school in Europe and in Germany above all. The relationship turned round in the course of time. American sociology became central. European sociology became peripheral. The far greater degree of institutionalization of sociology in the United States, the large scale of its output, the ascent of the United States to a condition of academic centrality (and the greater power and prominence of the United States outside the intellectual sphere), and the formation of an, to some extent, international sociological culture have all contributed to change the direction of the ecological process.

This process has been much aided by the increased institutionalization of sociology in teaching and research in Europe. Institutionalization increases receptive power just as it increases radiative power between countries as well as within countries. Once sociology reduced the freedom of its exponents to believe what they wished and to call

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sociology whatever they liked to think sociology ought to be, American influence was bound to increase. The mere organization of regular courses of study required syllabi and textbooks, and this increased the likelihood that the voluminous American literature would be brought in. Of course, the cessation of sociology in Germany from 1933 until the last years of the 1940's was responsible for a vacuum in the literature of sociology in the German language, and American literature was available to fill the vacuum. (The American occupation of a large part of Western Germany and the easy physical availability of American sociological publications also helped to bring about this condition.) But whereas the teaching of sociology in Germany has been extensively institutionalized, sociological research has not had the same experience. As a result, the proportion of empirical research in the total body of sociological publication is fairly small. One is struck with the very considerable German knowledge of American research and the contrast with the small amount of research done in Germany. German sociological monographic series, for example, *Göttinger Abhandlungen zur Soziologie, Frankfurter Beiträge, Bonner Beiträge zur Soziologie, Soziologische Gegenwartsfragen,* although containing more reports on field research than was the case with comparable series during the Weimar Republic, still contain many definitional, programmatic, and *dogmengeschichtliche* works. The same is true of the articles in the *Kölnische Zeitschrift für Soziologie.*

In France, on the other hand, there has been much more production of empirical research, thanks to the generosity of the *Conseil National pour la Recherche Scientifique* (CNRS). The point of reference, however, is, as in Germany, American research.

The institutionalization of sociology has made for greater intellectual interdependence. It has become necessary to master "the literature" if one is to teach or do research nowadays. A sociologist cannot live from himself nowadays in the way in which Simmel did or even Durkheim or Tarde or Fritz Sander or Haserot. This link with the outside is not random in direction or terminus.

Ecology: Within National Sociological Communities

I shall confine my observations of the ecology of sociology to the United States and, more particularly, to the relations between dominance and institutionalization. In the 1920's and 1930's the University of Chicago was the center par excellence of sociological studies, although in the 1930's it was existing on the momentum of the 1920's. As the leading place for postgraduate studies in sociology, the University had continuously present a group of mature and concentrated stu-

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dents, each of whom was engaged in a piece of research—almost always field research—under the direction of two men who were in a close and assymetrical consensus, Professors Park and Burgess. What they taught had been in principle codified—rather primitively by present-day standards—in "Park and Burgess." Intensive instruction was amply provided in seminars and informal lectures, as well as in numerous individual consultations between teachers and students. The institutions of the term paper and the seminar paper were in full operation. Each student regarded it as his obligation to produce such a paper, and the teachers regarded it as theirs to supervise production and scrutinize results. Relations were established with various local civic organizations to facilitate the students' entry into the field for direct observation, interviewing, and the collection of documents. There were certain adjunct institutions to which students could be attached for their research (and sometimes employment), such as the Institute for Juvenile Research, the Chicago Crime Commission, and the Chicago Real Estate Board. For the most successful there were the rewards of publication in the journal conducted by the department, the *American Journal of Sociology* or, even higher, inclusion of a finished work in the Chicago Sociological Series. The journal was the official organ of the American Sociological Society and the only important national journal on the subject. Relatively few attained publication in the monograph series, but its existence provided a number of standard-exemplifying works, and it added to the awareness of membership in a central institution. For many years the seat of the American Sociological Society was the University of Chicago.

In nearly every important state university of the Midwest and Far West, sociology was Chicago sociology. The departments were headed by Chicago graduates. It was to Chicago that they turned for new members, and it was to Chicago that they sent their best graduates for further study. There were other departments of sociology in the country, most notably at Columbia, Michigan, North Carolina, Brown, Pennsylvania, and Yale, and they had their small areas of hegemony to which their graduates were appointed. There were distinguished sociologists at some of these institutions—Ogburn, Lynd, and MacIver at one time or another at Columbia, Cooley at Michigan—and some valuable monographs based on dissertations were produced. None of these lesser centers had the common standpoint, continuity of effort, and liveliness and intensity of an intellectual community which Chicago had, and that made the great difference.

Chicago maintained its position as center of sociological studies not only because of the intellectual power of some of its staff members, but also because it was more institutionalized; it produced more work

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with a common stamp and the quantity as well as the quality aroused attention and respect. This, in turn, enabled it to draw outstanding graduate students whom it trained and then sent out to maintain the system of Chicago dominance.

Although Chicago sociology was embedded in a more ramified corporate network, its institutionalization was in one important respect like that of Durkheim's school. It depended very much on one major intellectual personality at a time. For a while, mainly over the years of the First World War, there were two intellectual leaders, but when Thomas' academic career came to an inglorious end, there was only one. When Robert Park withdrew from Chicago toward the end of his life, preferring to spend the last years at Fisk University with Charles Johnson, his old pupil and then president of the university, Chicago sociology faltered. It lost its focus on urban and ethnic studies; new persons, changes of interest, and a diminution of intellectual authority within the institution in consequence of intellectual and then personal disagreements weakened the sense of centrality.

Perhaps more important was the fact that the fundamental ideas of Chicago sociology were not clarified, extended, and deepened. With Park's departure, the analysis of ecological processes diminished; the studies of communities also declined, so that there was no development of the concepts which would have permitted more refined studies of the same kinds of situations which had once been so productive. Burgess' family adjustment studies with a predictive interest, Ogburn's statistical time series of various social phenomena and concentration on the improvement of techniques of quantitative analysis, without a persistent substantive interest reduced the radiative and attractive power of Chicago as a center. Ogburn's interest in the quantitative description of trends and his simplistic and undifferentiated concept of "cultural lag" was not fitted into microsociological analyses of situations which could be studied by methods of participant-observation. The inchoate global, macrosociological interests of Park found no forceful reformulation in a way which could give coherence to the work of the department. Sociology persisted because it was well institutionalized—unlike its Parisian counterpart—but the orientation provided by Thomas and then by Park became powerless. There was no successor to Park at Chicago who was strong and expansive enough intellectually and temperamentally to continue the Chicago tradition and to assimilate into it the new problems and modes of thought and inquiry which began to emerge in the years just before the outbreak of the Second World War. Intellectual incapacity to amalgamate new and diverse problems and knowledge furthered disaggregation from temperamental uncongeniality. The loss of a center within the department accompanied the loss

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of centrality in the national sociological system. The relationship was a circular one.

Not only internal developments reduced the centrality of Chicago. New centers were emerging in the East, places which had never been subcenters of Chicago. Columbia and Harvard were of special prominence. Columbia, by the early 1950's, markedly surpassed Chicago as a center, for several important reasons. One of the most obvious is that Columbia possessed two major intellectual personalities, Robert Merton and Paul Lazarsfeld, who combined what was most "needed" in sociology—ingeniously contrived techniques of survey research with interesting, quite specific, substantive hypotheses. A second factor, closely connected with the first, was the formation of a superior form of institutionalization at Columbia in the Bureau of Applied Social Research. The scale of operation, the interests of the moving spirit (Lazarsfeld), and the shape of development of sociology at the time required and permitted a higher degree of formalization of procedures—intellectual and organizational—than temperament, capacity, the primitive state of the subject, and the small scale of the operation had fostered at Chicago. The routinization of training facilitated the routinization of research organization and research procedures, at the very moment when new techniques of observation—sample surveys—and new techniques of analyzing data were brought into sociology from market research and statistics. The research for which Columbia graduate students were trained could be done without personal inspiration: it was easily reproduced and multiplied. This increased the proliferation of the Columbia center, at a relatively high level of technical competence, and encouraged the diffusion of its procedures and mode of thought to many nonacademic institutions which sociology had not penetrated before. It also made Columbia a national and international center for sociology. (Partial subcenters were formed at Chicago and at Berkeley —wherever, in fact, a "research facility" was created. The Survey Research Center at the University of Michigan was an exception to this; it did not derive from Columbia.)

Another reason for the emergence of Columbia as a center was the intellectual style of Robert Merton. A product of Harvard, where he had studied under Parsons and Sorokin, Merton had a very wide sociological culture. Unlike Parsons—who possesses a profound vision of society which he repeatedly struggles to articulate—Merton's view of society is more orderly and more adequately articulated, but his exposition is more fragmentary and eclectic. Merton's espousal of "middle range theories" is less penetrating than Parsons' efforts at a "general theory," but they are easier to apprehend and therefore more effectively taught. They are easier to formulate in a testable way, they seem to involve no *weltan-*

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schauliche commitment, and they are more congenial to institutionalized routines. All these factors together resulted in a larger production of works bearing a common stamp and of persons capable of producing more such works in the future. This led to a wide diffusion of Columbia sociology and it generated—more on the level of procedure than of substance—a consensual element in sociology which will probably endure.

Harvard's centrality, based on traditions transplanted from Heidelberg (Max Weber) and Vienna (Freud), was attributable to a new pattern of institutionalization. There, under the leadership of Talcott Parsons, Henry Murray, and Clyde Kluckhohn, a deliberate attempt was made to integrate the theories of social structure, culture, and personality. The teaching program was adapted to this conception of the subject, but the research training program did not keep up with it. The Laboratory of Social Relations never became the intellectual factory and drill ground which the Bureau of Applied Social Research became shortly after the end of the Second World War. It became the administrative sponsor and home of a variety of investigations which, to a greater extent than at Columbia, were small-scale projects which required neither an intricate division of labor nor a thoroughgoing routinization and uniformization of procedure. It never became the solidary collectivity with an identity of its own which the Columbia Bureau succeeded in becoming and, even before the death of the gifted Samuel Stouffer, it became more of an administrative name and rather less of a corporate intellectual reality. Although valiant efforts were made to create it, Harvard lacked a high degree of consensus among its central personalities such as was possessed to such a degree at Columbia. As long as the center at Harvard was a triumvirate with an outward appearance of unity, it presented a powerful force to the outer world of sociology. Nonetheless, it was in fact a center consisting internally of several relatively noncommunicating segments—and this reduced its capacity to impose itself effectively on the subject as a whole. At the same time, it should be stressed that each of the major segments was a powerful intellectual personality—Parsons, Murray, Kluckhohn, Bruner, Stouffer, and Homans—each of them in one way or another a forceful generator of ideas and works. A high degree of consensus among them might have swept the field. (It did pretty well as it was!) It also lacked what Chicago in the 1920's and early 1930's possessed, namely organs of publication and stable extra-academic institutional links with the local community for research and training purposes.

On the other hand, it had in Professor Parsons a motor force of great power; his continuous and pervasive productivity spread his influence over the country into the by-this-time numerous subcenters

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with highly institutionalized training provision for postgraduate students. The role of one particular publishing enterprise—The Free Press—which became the main source of sociological nutriment for the growing number of graduate students and young teachers from the end of the Second World War until about 1960—provided Professor Parsons with a surrogate for his own journal. His numerous, widely scattered essays were concentrated into a few easily available volumes. These essays enabled a new generation to acquire the underlying disposition of Parsons' theory as it was manifested in confrontation with a wide variety of particular problems. The enhancement of the prestige of Harvard University as a whole coincided chronologically with the prestige of the Department of Social Relations, which was largely the prestige of Talcott Parsons—although not exclusively so. This attracted to Harvard a group of gifted students.

The sociology department of the University of California at Berkeley also should be mentioned in these observations about the emergence of new centers in American sociology. The University of California at Berkeley was the last great American university to establish a department of sociology. It came into existence after Chicago, Harvard, and Columbia were well established with dominant traditions of their own. Berkeley could not recruit its staff from within, except at the level of its youngest members; it had to recruit from other universities. For example, Lipset trained in Columbia under Merton and Lazarsfield, Blumer at Chicago, Bendix at Chicago, Selznick at Columbia, Kornhauser at Chicago, Kingsley Davis at Harvard, Lowenthal at Frankfurt. The size of the staff, the size of the student body, and the amplitude of resources all made for centrifugality. The high degree of institutionalization coexisted with a diversity of standpoints, so that the department gained eminence as an aggregation of outstanding and productive sociologists whose works drew the attention of the country and the world, but it lacked the coherence which Durkheim, Park, and Parsons gave to their respective circles of colleagues, collaborators, and students. We know from Chicago in its earlier years and from Harvard and Columbia in the several decades after the Second World War how effective a unitary center created by one dominant or two strong consensual figures is. It remains to be seen whether a pluralistic assemblage of eminent figures can be equally effective. Thus far, Berkeley has not shown its capacity to create a *Nachwuchs* which will renew the department while unifying it or to produce a body of graduates who will diffuse its "line" more widely in the United States.

Sociology at the University of Michigan had been gently influential through the writings of Charles Cooley ever since the turn of the century. Both Thomas and Park had assimilated Cooley's ideas about

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the primary group into their own dominant traditions. There had not, however, been an independent department of sociology at Michigan until 1930, when Roderick McKenzie, a Chicago product and a close collaborator of Park, came there from the University of Washington. The Department of Sociology at the University of Michigan became one of the major departments of the country, partly through the extension of ecological studies and partly through the establishment of the survey research center there. The latter was an important step in the institutionalization of training and research at Michigan; in its ecological and related studies, Michigan continued and developed the Chicago tradition with the aid of many recruits from Chicago.

The Department of Sociology at the University of California at Berkeley was the last of the five major centers to emerge. The Berkeley department is not associated in the minds of those who live in the international sociological system with any particular ascendant individuals. Can a center be effective if it consists of an un-unified plurality? The question about the University of California at Berkeley might be academic in the pejorative sense because the disorders in American universities, of which Berkeley is at the forefront, might so disaggregate and demoralize an institution that it loses its capacity to operate effectively on its students. The functions of a center might still, however, continue nationally and internationally to be performed through the other channels of communication which are integral to institutionalization, namely, learned journals, monograph series, and books. But one essential organ of institutionalization is the face-to-face encounter of teachers with students. It is through these encounters that the *Nachwuchs* is formed and that ideas are generated, tried out, and selected from among a variety of alternatives.

The absence of a systematically trained *Nachwuchs* does not mean that an intellectual subject cannot develop: it did so in the heroic age of science prior to the formation of the modern university. The absence of a systematically trained *Nachwuchs* means that the pressure of tradition is lightened. The development of an intellectual subject under such circumstances was more a matter of individual genius than it became with the transformation of universities into institutions of research and training. Such a situation might slow down the rate of development of the subject in question by diminishing the density of work in it.

Sociology has now had its organizational, economic, and demographic revolutions; it has not yet had its intellectual revolution. The question may be asked whether a unified center is possible under present circumstances in the world of learning.

The proliferation of sociologists and of sociological works in the past quarter of a century resulting in great specialization in a subject still

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deficient in fundamental theory might mean that it will disintegrate, losing the small coherence which it has gained over the past three-quarters of a century. Sociology might break up into specialisms such as those which exist now in the physical and biological sciences, without being able to draw on a basic or fundamental science like chemistry or genetics of physiology.

The constituent elements of the traditions of present-day sociology have been in a process of selection and coalescence as a result of the work of Weber, Durkheim, Parsons, et al. This coalescence is still very imperfect. (It can never be perfect and if it did become perfect it would come to a complete halt.) Greater coalescence than it possesses at present is, however, a necessity, not just out of aesthetic or architectonic considerations, but also because until there is such an improvement the interrelations of the different phenomena in society will be too poorly understood. Thus the multiplication of sociological works and persons might render more difficult the entry into ascendancy of a more complex and comprehensive theory because it renders more difficult the ascendancy of a theorist whose mastery of the results of specialized study would be great enough to call forth the respect and adherence of the practitioners of the diverse and numerous specialisms.

Sociological Traditions: Selection, Rejection, Coalescence

If one takes up the compendious work of Sorokin, *Contemporary Sociological Theories*, which presented a gallery of the stock of sociology of the fifty years which lay across the turn of the century, one is struck with how few of the names cited are known today by any but sociological antiquarians. Weber, Durkheim, Pareto, Park, and Thomas are there; so are Floyd Allport, Otto Ammon, Emory Bogardus, Stuart Chapin, Filipo Carli, E. de Roberty, Charles Ellwood, Franklin Giddings, Maxim Kovalevsky, E. A. Ross, Gabriel Tarde, William Graham Sumner, Alfred Vierkandt, Leopold von Wiese, L. Winiarsky, and Sorokin himself, who is cited more than any other two authors. What has become of these authors and of the ideas they espoused? They have fallen by the wayside.

Some of them were weak intellectually and their ideas did not recommend themselves to critical contemporaries. Some of them were in the wrong places; their books were the only ways in which they spoke to their contemporaries; they had no students or no students who had the ability, received the inspiration, and learned how to press further with what they had received. They might have been in the wrong countries and written in the wrong language. Some of them were known and respected figures in their time, which was contemporary with Durkheim, Weber, and Park, but the world turned away from them.

There were many ideas floating about in sociology in the latter part

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of the nineteenth century. Practically all of them were influenced in one way or another by Hegel, Comte, and Darwin. Evolutionary and biological ideas were common. The major idea was one of historical stages of human society from simple to differentiated, from traditional to rational, from unenlightened to enlightened, from societies with tool-based technology to societies with a machine-based technology using artificially-generated power, from loosely integrated societies to closely integrated societies. The Darwinian evolutionary viewpoint fitted into and rendered more plausible this theory of stages of development; the prestige of Darwinism also activated a tendency to analyze social structures and their functions in society in the categories appropriate to the study of biological organisms. (This type of sociological analysis was submerged rather early and left little trace.) The Darwinian influence was also present among those sociologists who, impressed by the competition of species, stressed the importance of the competition and the conflict of races as a central phenomenon in society. Others, also impressed by the idea of natural selection, were concerned with social selection and hence with the social origins and the presumed biological heredity of the members of various strata, above all, the leading strata in contemporary western societies. Many studies were intended to demonstrate the biological transmission of socially relevant characteristics. Much of the substance of these propositions no longer has any place in the effective tradition of sociology.

Certain variables in this set of beliefs have survived—for example, the study of elite-composition and the social origins of particular professions—but the matrix of propositions has fallen away. And what has survived has not been comfortably assimilated into the presently effective tradition of sociology.[30]

A few words should be said at this point about Marxian sociology, which has so many self-alleged proponents at present. "Marxian sociology," such as it is, is an amalgam of Hegelian evolutionism into which has been inserted a conception of conflict drawn from Darwinism[31] and a conception of interest generalized from the eighteenth-century conservative conception of the "landed interest." This results in something different from the mere substitution of "class" for "group"—ethnic, national, territorial—which was developed by Ratzenhofer and Gumplowicz, and it is also different from that type of conflict which arises from contention for the possession of scarce objects. The prevailing conception of society derived from Weber and Durkheim allows, despite the assertions of the critics of the "consensus-model,"[32] ample place for conflict, but it can make no place for transcendent interests which are independent of what is desired by actual human beings in situations of scarcity. This historical metaphysics of transcendent interests could obviously not find a place in

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a tradition which received one of its possible promulgations in *Towards a General Theory of Action.* The same may be said about the distinction between the superstructure and the infrastructure and the causal primacy of the latter. The distinction derives from a conception of human action which is alien to the more realistic, synthetic tradition contained in much of contemporary sociology.

Thus a considerable amount of the Darwinian or biologistic and the metaphysical evolutionary—Hegelian in fundamental pattern but rendered "scientific" and therefore acceptable by Darwinian idiom—could not be fused with the traditions which prevail in contemporary sociology. They have either become extinct or they lead a restless uncomfortable existence recurring intermittently at the margins of sociological culture, functioning to criticize but incapable of positive development in a culture which is committed to empirical theory—however abstract—and empirical research—however imprecise.

Of the Comtian and the Hegelian concepts of social evolution, certain very important elements have survived. The conception of society as a moral order in which discrete individuals are bound together in collective actions of various sorts by common images of themselves as parts of that order and by common beliefs defining themselves and their obligations to the collectivity is one of the chief precipitates of the coalescence of the various intellectual traditions of the nineteenth and eighteenth centuries. From Rousseau's conception of the collective will, through Hegel and the German theorists of the *Volksgeist,* from Comte's portrayal of the morally disintegrated condition of the then contemporary society in its "critical," skeptical state, and from Marx's conception of the dissolution of the moral bonds of modern bourgeois society emerged a variety of conceptions of the significance of beliefs—about the ultimately valuable—in the regulation of individual conduct and in the determination of the degree of integration of society. Max Weber's typology of legitimate authority was developed from the presupposition of the significance of beliefs and of a consensus of beliefs about ultimate things; the moral state of society which engaged Durkheim's interest, the conception of *Gemeinschaft* and *Gesellschaft* in Tönnies, the conception of the moral order in Park—all these beliefs about the scale and significance of a consensus of moral beliefs in the working of society have been the products of a confluence of traditions into which Hobbes, Rousseau, Hegel, Marx, Lazarus and Steinthal, H. S. Maine, et al. have contributed. The Marxian concept of bourgeois society unbridgeably divided into competing and conflicting classes; the Simmelian conception of urban society differentiated by division of labor, sectional interests, and individuation; Tönnies' conception of modern bourgeois society—*Gesellschaft*—which resembled Hobbes's picture of the state of nature where a

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relationship of *homo homini lupus* prevailed all implied a great concern with the extent and the significance of a consensus of moral beliefs. The problem was: to what extent do individual men act on the basis of conceptions of themselves, their rights and obligations, in ways which minimize or aggravate the conflicts between individuals and between groups? Consensus became the main variable of analytical (or theoretical) sociology. Even those authors who claimed that modern societies were atomized had this point of departure in an image of a consensual society: they regarded the dissensual character of modern societies as the phenomenon to be accounted for. The very definition of modern society involved placing the category "consensus/dissensus" at the center of attention.

The struggle for existence which was one variable of the sociological Darwinism and the basic theorem of the *Rassenkampf*-sociology could be assimilated into the emerging sociological tradition because it constantly raised the problem of "consensus-deficit." In Ratzenhofer and Gumplowicz, the conflict of races was given; it was not problematic. In the analytical sociology, which became ascendant in the work of the European and American founders, conflict was not given; it became a variable to be explained. Its converse—consensus—was likewise not given. It, too, had to be explained. Research on the adjustment of immigrants or family disorganization, ethnic conflict and segregation, suicide, *anomie*, sect-formation, the role of charismatic authority and of rational-legal authority were all attempts to cope with this fundamental problem. Features of other ideas which could not be assimilated were discarded.

Certain other important ideas of contemporary sociology were possible only within the context of a concern about the moral beliefs of a society. The idea of the charismatic or the sacred was an extension of this conception. The delineation of the properties of bureaucratic authority and the assessment of the role of bureaucracy in modern societies were elaborations of the conceptions of *Gesellschaft* in Tönnies and of capitalistic society in Marx. The same obtains for the conceptions of "mass society," "modernization," and "organizations" which assumed such prominence in subsequent sociological studies; they were developed out of the fundamental conceptions of the sociology formed by the continental founders of the subject.

Even before it became anywhere nearly as institutionalized as it has been for twenty-five years, this tradition was in the process of gaining ascendancy. It was, as Professor Parsons points out in *The Structure of Social Action*, contained implicitly in the three main European traditions of idealism, positivism, and utilitarianism, and it was elicited from potentiality by the powerful intelligences of the major European theorists. The American theorists of the founding generation were not of equally

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powerful theoretical intelligence, but they too—Thomas and Park—came upon the essential. Neither Thomas nor Park ever succeeded—nor did they try in any large-scale effort—in discarding the elements of incompatible traditions. In Park's case, fragments of Darwinism remained in his ecological ideas. Thomas, indeed, after his exclusion from the University of Chicago, turned away from not only his sociological work but also the framework which he had developed in it and which brought him near to Weber and Durkheim; he was attracted by an eclectic behavioristic psychology which was alien to what he had believed previously. There is some reason to believe, however, that after this digression he began to find his way back again, but no published work ever announced his arrival.

Somehow, despite its very scanty institutionalization in Germany, despite its submergence in France, despite its numerous competitors in Great Britain, the now dominant tradition grew and penetrated. With institutionalization and the emergence of new centers in which a more stringent formulation of the tradition was presented, it finally triumphed over competitors—not totally—in the United States. However, after the United States became the center, the reinterpreted and modified tradition then spread back to Europe, where, despite resistances, it established itself in all the main European countries in a way in which it had never done before when Europe was itself the center from which the tradition emanated.

To what extent is its wide diffusion and dominance attributable to institutionalization?[33]

The turning of direction of a tradition, the amalgamation of elements of several traditions, is a creative action. It is not an institutional action. It is the work of an individual mind and it can and has been done under conditions of very rudimentary institutionalization. As a matter of fact, it might even be said that institutionalization, as important as it is for consolidation, multiplication, and diffusion, can be something of a hindrance to the turning and amalgamation of traditions. Max Weber had the advantage of an uninstitutionalized sociology in his time. He had studied law and economics; he came on the study of religion without institutional supervision. Durkheim had not studied sociology as an academic subject. Park studied it under conditions of very scanty institutionalization, and Thomas studied it in Chicago when the subject still scarcely existed even though an institutional structure had been created for it.

A tradition can give birth to a product at one stage of its existence which it could not produce at an earlier time. Its potentialities might be presumed to have a sequentiality and it is quite possible that this stage had been reached at the end of the nineteenth century, so that in a num-

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ber of countries persons who had assimilated positivism, utilitarianism, and idealism began to produce results which had marked affinities with each other. Those who produced these results entered into practically no contact with each other. It was for the next generation of those born after the beginning of the new century, with Parsons in the forefront, to draw these separately generated approximations to a tradition together into a more explicit and coherent pattern. The subsequent history of the consolidation and diffusion of the tradition is very much a history in which the institutional system of sociology has played a crucial role.

Exogenous Traditions, Endogenous Traditions, and Exogenous Stimuli

An intellectual discipline exists when a number of persons believe themselves to possess an identity defined by the common subject of their intellectual concern and when many or all of the problems which they study are raised by or derived from the tradition—that is, the body of literature and oral interpretation produced by those who regard themselves as practitioners of the discipline. An intellectual discipline is an academic discipline when it is taught—that is, discussed and investigated—through academic institutions which bear the name of the discipline or something akin to it. When its members publish works in organs bearing the name of the discipline, a discipline has an intellectual and social structure.

In its early years, while its proponents were still struggling to establish their intellectual and academic dignity, a relatively common activity of sociologists in Germany, France, Great Britain, and the United States was the delineation of the field of sociology, its demarcation from other already existing academic disciplines, and the laying out of its internal subdivisions. Sociologists at that time had no social structure for their discipline in most instances, and so they sought to define it by its intellectual properties. This was extremely difficult to do because the ancestry to which they had claim was not universally acknowledged by nonsociologists and because there was little in the way of substance which they could invoke in self-legitimation. Therefore, they argued in principle and made claims for the future. This activity has now practically vanished, partly because most sociologists have come to accept a body of problems, ideas, and procedures as sociological, despite unclear boundaries and heterogeneity of subject matters, problems, and approaches. The large body of literature containing these problems, ideas, and procedures, which appears in sociological journals, is reviewed there, and is produced by persons called sociologists, fortifies this sense of sociological identity. Sociology is also now recognized as a subject by the educated

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public inside and outside the universities, and this too helps to define the boundaries of the subject and of the profession which cultivates it or at least allays apprehensions about its legitimacy.

The definition of sociology by the social structure of sociology and by its corpus of works has a retroactive effect on the construction of the sociological tradition. While traditions work forward in time, the construction of a legitimatory, inspirational tradition is a temporal movement in the reverse direction. Tradition is not, however, a mythological construction, although it has some of the functions of mythology. The works and ideas which have been admitted to a tradition—that is, those which are regarded as part of it—set the problems. They can set problems in a situation in which the ostensibly valid part of the tradition is the work which has most recently been published, although the problems of the recently published works were set earlier by a major work which might go back many years. Alternatively—although the situation is not so different—current work might constantly refer explicitly to a major work done many years earlier. Sociology is at present a mixture of these two relationships to "its own past." The significant fact is that sociology now has a fairly consolidated central tradition and a number of less dominant traditions. Some of these less dominant traditions are in a sense countertraditions. These countertraditions are mainly polemical rather than substantive. Behaviorism as exemplified in the work of George Lundberg was one of these; the self-designated "critical sociology"—an evasive name for highbrow Marxism—is another. Both of these countertraditions, insofar as their adherents produce substantive works, turn out to have little nutritive value intellectually and their adherents turn out to be dominated by the prevailing central tradition in their selection of problems and in their selection of the major variables with which they operate. Allowing for the ambiguity and lack of rigor of practically all sociological work and the large degree of freedom of interpretation permitted by the poor data and the disjunctiveness of detailed empirical research, even the interpretations offered by adherents of the countertraditions turn out to be little different from the interpretations offered by the adherents of the dominant tradition.

The subsidiary traditions, on the other hand, are not polemical. They have a more positive relationship to the dominant tradition. They are viewed sympathetically by adherents of the dominant tradition. They have become tributaries into the dominant tradition, even though they arose outside it. Comparative religious studies from Spencer onward are of this sort. Thanks to the work of Weber and Durkheim, the results of comparative religion have found a place in the dominant tradition, although comparative religious studies still constitute an autonomous field of work with a strong central tradition of its own. Modern economic

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analysis is another. At one time, in the writings of the European founders, economics up to the time of Mill and Senior found its way into sociology. Talcott Parsons succeeded in bringing in Marshallian economics too, but his later efforts to assimilate the Keynesian development from neoclassical economics have been less successful.

Animal and plant ecology and the economic theory of location (von Thünen) also coalesced with the central sociological tradition as it was emerging into dominance; it faded as the center shifted from Chicago. Since these subsidiary traditions were never persuasively and explicitly integrated in an authoritative work by Park or his associates and protégés, they became submerged or disassociated as far as sociology was concerned.

The psychoanalytic theory of personality is another tradition which coalesced with the central sociological tradition. The particular propositions of psychoanalysis might not be true; the mechanism of the Oedipus complex might not be as psychoanalysis has described it; the same can be said about the genesis and mechanisms of aggression, conscience, anxiety, repression. Nonetheless, the indication of these variables in the coherent pattern in which psychoanalysis set them forth has been assimilated into the sociological tradition. The naming and description of these phenomena has made sociologists more aware of them, more realistic in their perception of them, more able to assess their magnitude and to estimate the probability of their occurrence under certain conditions.

There have been other coalescences of the sociological tradition and intellectual traditions external to sociology. Traditions from various currents of philosophy—for example, from phenomenology, from legal studies, from anthropological study of social structure and cultural patterns—have flowed into the sociological tradition, sometimes becoming well integrated conceptually, sometimes less so. Sometimes the traditions which reach toward sociology are contemporaneously active. Sometimes they have been dormant even outside of sociology for a long time. The most striking instance of the latter is the revival of Tocqueville's ideas about the consequences of equality and the delineation of the structure of an equalitarian society. The revival of Tocqueville occured first in political science after the Second World War and it moved into sociology with the increased attention to the structure of postwar society. The account of Tocqueville as a sociologist in Raymond Aron's *Les Etapes de la Pensée sociologique* has now made Tocqueville's ideas retroactively into a constitutive element in the contemporaneously effective tradition of sociology.

Sociology and comparative religious studies, sociology and economics, sociology and ecology, sociology and psychoanalysis, sociology and cybernetics, sociology and the theory of administration—all these coales-

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cences with their simultaneous modifications of the content of sociology today and of the image which sociologists have of their own past are all coalescences of substantive traditions, of traditions which have grown out of the sociological tradition endogenously and of intellectual traditions which have developed outside of sociology. But they are all substantive traditions. Sociology is not, however, merely a body of substantive assertions: it exists within the more broadly embracing tradition of discourse which requires proof of its assertions. In consequence of this sense of intellectual obligation, the substantive traditions of sociology have increasingly, although unequally, affiliated themselves to traditions of observational organization and technique.

There is a long and many-streamed tradition of sociological procedure which has become more and more coalescent, in practice and in retrospect, with the substantive traditions. Paul Lazarsfeld has created a retrospective map which goes back to Petty and the English demographers of the seventeenth century. More recently he has established Quetelet as a precursor. Philip Abrams has now presented a coherent account of the course of the "statistical" tradition. Every new step in sociology toward a more rigorous procedure of empirical research adds a new set of ancestral deities to the pantheon of the sociological tradition. These, in turn, add their force to the movement of coalescence.

The initiative for the coalescences of endogenous and exogenous intellectual traditions has always come from within sociology. In that sense sociology has become a realm of its own with its own center of gravity, its own discriminatory powers. This is tantamount to a continuing expansion of sociology in its subject-matters, in the differentiation of its analytical scheme, and in its corresponding interpretative hypotheses; it also entails the increasing stringency of its procedural standards.

The tradition which is regarded as sociological is heterogeneous, not just in its substantive focus, but also in its implicit assessment of the relative intellectual dignity of different strands. It is heterogeneous because of the imperfections in the assimilation or integration of the many coalescent subsidiary or tributary traditions. The various strands of the sociological tradition are not shared equally by all sociologists. Not only are there the limitations imposed by the specialization of knowledge, but there are also differences in interest and esteem which mean that any particular sociologist will be more sympathetic to one strand or family of traditions and more hostile toward certain others.

Sociology does not, however, live from its intellectual inheritance alone, and its exertions are not confined to the refinement and enrichment of that inheritance by the study of the problems which that inheritance offers and by the incorporation of new elements from exogenous intellectual traditions. Sociology does not just live within its primary

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institutional system, it also belongs to the larger world. Its subject matter is that larger world. Sociologists are members of social classes, ethnic groups, religious communities, and political organizations, and they share to some extent the culture of these social systems. They belong to their society and their generation and they share to some extent in the cognitive beliefs and evaluative attitudes of their society and generation. Sociologists are citizens in their own societies and they respond to the problems of their societies. The institutionalization of sociology has entailed training students for roles in that world. The institutionalization itself has been greatly furthered by the belief of those in positions of authority and influence in that larger world that sociology "has something to contribute" beyond its contribution to the improvement of the sociological tradition. Part of the institutionalization of sociology in recent decades has involved either the subsidy (or contracting) of research by university research workers on particular subjects designated by the patron as well as the conduct of research within the corporate framework of public or private bodies concerned in a practical way with certain problems.

It has been claimed that sociology often chooses its problems and its subject-matters according to criteria of value-relevance and not just in terms of the problem-complex presented by the intellectual tradition. For example, the study of the Negro, as developed by the Chicago sociologists, has been asserted to be a response to the problems of urbanization and migration. But it was at least as important as a study of the processes of competition, conflict, accommodation, and assimilation. These categories had been developed by Park from his study of Simmel, Kistiakowski, and Gumplowicz, from his study of the work of a Danish ecologist, and from his own observations, both direct and historical. These seem to me to be still more or less the right categories in which to study the relations between groups, including those between whites and Negroes in the United States. Before the Second World War, the study of the Negro in America was further enriched by the introduction of certain approximately psychoanalytical variables, and a number of valuable monographs were produced along these lines. But at a time when the Negro question was not less urgent than it had been before, and for no good reason other than the relocation of the center of the sociological institutional system to a place where the sociological tradition espoused had not dealt with ethnic relations as a particular concrete subject-matter, the Negro ceased to be an object of study. This occurred practically at the time that Myrdal's vast synthesis of previous American—to a large extent, Chicago—research appeared. Why? Had the analytical problems involving competition, conflict, accommodation, and assimilation been so exhaustively solved that the subject no longer had anything

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interesting to offer? Or did the Negroes themselves cease to be a "value-problem" in the United States? Neither of these answers seems to me to be acceptable.

The decline in intellectual interest in the Negroes in the United States was a result of several factors. The first is that the new centers of sociological study—Harvard and Columbia in particular—had their own substantive intellectual traditions which did not include the Negro. In Max Weber, primordial things received little explicit attention: in *Wirtschaft und Gesellschaft* only one chapter of eleven pages is devoted to *ethnische Gemeinschaften*, and in the writings of Professor Parsons they received no attention until the 1960's when the Negro problem became urgent in the United States. I think that until relatively recently not more than two Harvard Ph.D. dissertations were written on Negroes. At Columbia, where several interesting dissertations had been written on Negroes, for example, *Sea Island to City*, in the 1930's, the subject nearly disappeared in the Merton-Lazarsfeld epoch. The eclectic analytical outlook of Professor Merton could certainly have been accommodated to almost any subject-matter and the technical virtuosity of Professor Lazarsfeld was likewise applicable to the study of race relations and to the study of Negro society. At Chicago, in the disorientation referred to earlier and under the impact of the Harvard and Columbia styles of sociological theory and research, interest in the Negroes was allowed to lapse. The old center lost its confidence in its own substantive tradition.

In the 1960's, when the "Negro problem" came forward with an unprecedented urgency, the study of the Negro was revived, but within a narrower framework and, in any case, a different one from that which guided the earlier studies. New methods of measurement of occupational discrimination, of the share of the Negro communities in the national income, and so forth, have been undertaken, but the social structure of Negro-white relationships as developed in the tradition of Thomas, Park, and Burgess, by Franklin, Frazier, Bertram Doyle, and Charles Johnson has not been taken up again. The "Negro subject-matter" of sociology might have survived the period from 1940 to 1960 if the Chicago tradition had received an authoritative theoretical formulation capable of demanding explicit incorporation into a new analytical scheme developing at Harvard. Since, however, it had not gone beyond fragmentary, only implicitly coherent, statements, its competitive power was not sufficient. Thus, despite its analytical affinity with the newest developments in the sociological tradition, its idiom was submerged and, with that, its particular subject-matter. Had its idiom survived in the new analytical scheme, its associated subject-matter would likewise have survived.

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The vicissitudes of the study of the Negro in the United States would indicate that the substantive analytical or theoretical tradition was not affected by exogenous, nonintellectual occurrences. The change in the substantive or subject-matter tradition was, in my view, to a large extent the function of the adhesion of particular subject-matters and traditions to particular analytical traditions and of changes in the institutional system of sociology (for example, the relocation of centers). The re-emergence of the Negro as a subject-matter of sociological research has indeed been a consequence of exogenous nonintellectual events, certainly of nonsociological events. But has the scheme of analysis been affected by extra-intellectual events? Rather it remains very much a product of the sociological tradition and of an expansion in the direction of sociology emanating from the tradition of economic analysis (see especially the work of Professors Gary Becker and Kenneth Arrow).

The study of "popular" or "mass culture" is another illustration of the interplay of endogenous and exogenous intellectual traditions and exogenous nonintellectual events. Tönnies, Simmel, Weber, Mannheim, and Horkheimer created the tradition, but it was the response to the National Socialist movement and its triumph in Germany and the great expansion of the study of the content of and "exposure" to mass communications which led to the increased attention to mass society in American sociology in the 1950's. Changes in the political culture of literary and publicistic intellectuals in the United States—a disillusionment with Marxism attendant on the identification of Marxism and Stalinism and disillusionment with the working classes as the agents of revolution and the "heirs of German classical philosophy"—made for a more active attention to certain elements of the central tradition of sociology. The nonintellectual events—National Socialism and mass culture—did not divert the tradition of sociology; they were simply assimilated into it.

The career of industrial sociology and the sociology of work is another instance of a subject which was developed in the framework of the central tradition of sociology—much influenced by Durkheim's ideas about *anomie* and Max Weber's ideas about bureaucracy—and supported by private business management. In the United States, but not in Great Britain, the subject has lost its glamour—although obviously not its intellectual or practical significance. American sociologists have gone off that subject.[34] They have gone on to organizational analyses, under the inspiration of Max Weber and Herbert Simon, and on to studies of the professions of medicine, law, and science. The theoretical or analytical scheme remains what is offered by the cultural tradition, enriched and differentiated by the new data. The change in particular subject-matters is perhaps more a product of the availability of financial support for the study of these subjects, the much greater prominence in public opinion

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of the learned professions and especially of the scientific profession, and the decline among intellectuals of the prestige of the working classes. Latterly in the United States, sociologists have returned to the proletariat. But not so much to the employed "respectable" proletariat as to the unemployed, the "poor," or what was called by Marx the *Lumpenproletariat*. This change is partly a function of changes in the focus of attention in public opinion and in intellectual circles and of the opportunities afforded by the availability of financial support for such research. The relocation of subject-matter interest is relatively recent, but thus far there is no evidence that it has affected the intellectual pattern of the central tradition.

In Great Britain, a large proportion of the much-increased volume of sociological research is concerned with the working classes and the poor. This testifies to the strength of the British intellectual tradition—largely pre-academic, deriving from the statistical surveys of the nineteenth and early twentieth centuries—as well as the availability of financial support for inquiries into such subject-matters. The power of this tradition in Great Britain has been so great that an analytical tradition such as has been preponderant on the continent and the United States has scarcely developed. Even there, however, the chief analytical achievements—T. H. Marshall's *Citizenship and Social Class* and the writings of Michael Young and David Lockwood—remain within the framework of the dominant central tradition.

What we see from these few sketchily presented illustrations is that exogenous nonintellectual stimuli have indeed played a significant part in bringing forward subject-matters into the center of attention of sociological research. They cannot, however, be said to have made a comparable impact on the analytical framework of sociology which is contained in the central tradition. This framework has extraordinary continuity and stability. At the same time, this framework remains vague and ambiguous in its major categories and has experienced relatively little differentitation or deepening over the past quarter century. The main innovations have come from the intrusion of exogenous intellectual traditions and these have not yet been systematically integrated into the dominant tradition.

The Form of a Sociological Work

A tradition can be influential in an academic discipline in several ways. In one way, the most recent period's output provides the immediate point of departure, the prior output having set the point of departure for the former, and so on, while the works of the relatively remote past—the past of the middle distance—are influential either through a

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similar point of departure function or through the constitution of a framework of concepts or variables and fundamental problems within which the works which constitute a sequence of subsequent points of departure are performed. The latter are the monuments of the subject, not merely honorific monuments, but rather effective ones. They are what Thomas Kuhn calls "paradigms." But they are not referred back to except on honorific occasions. What is living in them? What is relevant for the development of knowledge in the field is contained in the latest stage of the tradition, the detailed research which provides the immediate point of departure for the next future stage of the subject. A third way in which tradition can be effective is through constant recourse to the monument.

These three patterns of relationship of present work in a given subject to the tradition of that subject correspond to the forms of work in both the natural sciences and sociology.

The most common form of scientific work is the short research report or paper which states the hypothesis, locates the hypothesis in relation to previous research, describes the research arrangements or experimental procedure, reports the observations or experimental results, and proposes the modification of previous hypotheses or beliefs required by the results of the reported investigation. There are also synthetic, compendious summaries of existing theoretical and experimental work which intend not to establish new knowledge, but to consolidate and order what is known in a broad field. These might be monographs with an element of originality of interpretation of what has been demonstrated by research or they might be textbooks which summarize what students and research workers at a certain level of their scientific development are expected to know.

In sociology—parallel to the differences in procedures and intellectual structure between sociology and the sciences—works tend to take the following forms. Monographic treatises describe a particular subject-matter, for example, a territorially delineated sector of society, a group, a stratum, or a profession. These sometimes take the physical form of a monograph of 20,000 words or, more frequently, a full-length book of 100,000 words. (The latter often contains the former.) They give much descriptive data with analytical remarks and usually a general interpretation of the phenomenon in relation to its larger setting at the end. These monographs or monographs-within-books are usually directed at a problem (relationships between variables), although their ostensible point of departure is a particular subject—for example, the medical profession in a given area; a particular village; a process, such as social mobility within a given country. There are also monographs which seek to deal with problems. These begin with an attempt to ex-

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plain a major phenomenon—for example, specifically capitalistic acquisitive behavior as in Max Weber's *Protestant Ethic*, the causes of various kinds of suicide as in Durkheim's *Suicide*, or the effect of political campaigns on voting choice as in Lazarsfeld's *The People's Choice*.

Then there is the journal article (corresponding to the scientific paper), which summarizes the existing state of knowledge on a particular problem, formulates a hypothesis, presents the data which report observations made by the author or by others, and attempts to harmonize the resultant interpretation of the data with previous interpretations.

There are also essays which clarify, refine, and differentiate particular ideas (concepts, categories of processes, variables, and so on) without close reference to observations except for illustrative purposes.

There are theoretical treatises which present "all" the major concepts or variables which the author believes relevant to sociology as a whole and hypotheses about the behavior of these variables under different conditions. One might mention here works as various as Weber's *Wirtschaft und Gesellschaft*, Parsons' *Social System*, and Homan's *Social Behavior*. These theoretical treatises verge at their lower levels into textbooks which cover all the concepts of variables and present illustrative data. Such works as Kingsley Davis' and Wilbert Moore's *Society: An Analysis*, H. M. Johnson's *Sociology*, and Park's and Burgess' *Introduction to the Science of Sociology* are representative. Sociological textbooks usually contain much descriptive material from monographs and articles, but very little rigorously established, widely accepted knowledge—unlike textbooks in the sciences.

Finally, there are collections of essays of single authors, a form which has become relatively widespread since the end of the Second World War. Indeed, collections of essays have been among the most influential books of the past two decades. The essays have usually been analyses of concepts and elaborations of themes, rather than scientific articles.[35] Such collections are also found in the sciences, but for the most part they are monumental in intention, being the works of very distinguished figures, living or dead, and many of the papers published in them have already played a great part in the development of these subjects and have outlived their immediate usefulness. They have a different function from the collection of essays in sociology because of the longer life of the sociological essay.

The function of the books composed of sociological essays discloses a major difference between the characteristic patterns of the growth of knowledge in sociology and in the sciences. In the latter, the most important means of speedy, non-oral communication are the research paper, and more recently the preprint. The research paper in its

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original form is important in science; when reprinted in book form, it is likely to have only historical interest. The emergence of the preprint in science is the very opposite of the reprint in sociology of an essay in book form. The scientific preprint is required because of the speed with which the tradition is being modified or supplemented; the reprint of a sociological essay is a function of the continued dominance of the tradition, particularly the dominance of the monuments.

The sociological essay can be reprinted in a book because it has a long life, whereas a scientific paper must perform its function within a relatively short time after its moment of publication.[36] The difference might be accounted for by the much sharper focus of research in the sciences, by the more consensual perception and interpretation of the results of any particular piece of research, and by the more immediate response in the form of further investigation of the problems it raises.

The completion of a piece of research on science entails its publication because science is a collective possession—the possession of the scientific community—and knowledge which is not in the possession of the scientific community is not knowledge. Discovery, therefore, integrally entails publication. To launch on a campaign of discovery is to launch on a course which requires by its internal structure the publication of the end state of the act of discovery. A work of research which does not contribute to the pool of knowledge is a wasted effort and failure to publish is such a waste. If the delay in publication is long, then the results of research will usually be rendered out of date. It will have been superseded by the results of the research of someone else. This high probability of supersession is a function of the greater precision in formulation of problems and greater specificity and concentration of the focus on problems in the sciences.

There are relatively few instances of the very specific articulation of successive research papers in sociology as compared with any field of comparable scope in the sciences. This is partly a result of the much lower degree of specialization, and the much less differentiated division of labor in sociology. The less differentiated division of labor in sociology is in turn a result of (a) the much greater quantity of literature produced in any field of the sciences and (b) a closely related phenomenon, the larger number of persons working in any special topic in the sciences. But without the specificity of focus and the greater consensus about the crucial problem—which exists in the sciences as compared with sociology—the larger body of workers on a given problem would be of little consequence for the life span of a research paper or report. This greater concentration of minds on particular, specifically delineated problems results in the previously mentioned more rapid obsolescence of works in the sciences.

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The form of the work, then, has an intimate connection with the style of thought and procedure of analysis of a subject, as well as with the scale and the structure of its institutional system.

The Professionalization of Sociology

Whereas at the turn of the century there were very few persons who called themselves sociologists anywhere in the world and even fewer who made a livelihood from it, there are now many thousands who teach what is called sociology in university course lists and thousands who are employed by governmental bodies, independent research institutions, market research organizations, public opinion surveying organizations, industrial firms, hospitals, and military organizations to do research which they classify as sociological. Most of the world's colleges and universities now provide teaching in sociology and permit specialization in it as qualification for a degree. There are nearly one hundred journals in all languages which have the name sociology in their titles or subtitles. There are about thirty national professional sociological associations, and in some countries there are also more specialized professional associations which regard themselves as covering a section of sociology. Sociology has become a subject of a learned profession.

One consequence of this professionalization is a movement toward homogeneity. National differences still exist among the dominant types of sociology—theoretical inclinations, subject-matters of research, preferred techniques,—but there is also a large common sociological culture, just as there is, despite specialization, within each country which has many sociologists. It is true that sociology in the English language enjoys the advantages of an asymetrical accessibility. Sociologists everywhere read what is written in English by sociologists, but sociologists in the English-speaking world read less of what is written in most other countries, even if it is published in English (as are *Acta Sociologica* and *Sociologica Neerlandica*).

Does professionalization—the triumph of institutionalization—entail the isolation of the sociological tradition from other intellectual traditions, academic and nonacademic? Not necessarily. Linguistically, sociology in the English language has shown the potentiality of isolating itself through ponderous and inexact terminology and stylistic barbarism, but the isolation has not in fact occurred. The curiosity of the educated classes outside of sociology has grown as their stylistic intolerance has diminished. Perhaps they do not know any better, perhaps they do not mind, but whatever the explanation, sociology has not been rendered inaccessible to nonsociologists by reason of its execrable literary quali-

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ties. It is not even likely that the mathematization of sociology which is in prospect—in some measure at least—will isolate sociology from the increasingly mathematized educated classes.

The accessibility of sociology to nonsociologists is, however, of less interest to my concern with the growth of sociology than is the accessibility of intellectual traditions outside of sociology to sociologists. If the sociological tradition in its present protoscientific condition were to become isolated from other intellectual traditions, much harm would result, but I think that the danger is not great. For one thing, the pattern of the dominant sociological tradition is open; its toughness and persistence is not a function of its logically rigorous coherence. Its openness is partly a product of the particularity of the central problems with which it began and its excessive generality. It stands in much need of, among other things, a frequent confluence from other intellectual traditions. The large number of sociologists and the diversity of substantive things studied by sociologists guarantees that at least a small proportion will include within their knowledge developments of intellectual traditions other than their own. A small proportion of sociologists, coming from other intellectual fields or curious about what goes on in them, will bring into sociology new subsidiary traditions. There is, furthermore, despite the self-congratulatory attitude of sociologists toward their constitution as a profession, enough discontent with what they have inherited to render them receptive to innovations. The institutional structure of departments of sociology, the competitive relations between centers, and the emulative relations between centers and peripheries are likely to forestall the growth of Byzantinism in sociology.

The Dominance of Tradition and the Growth of Sociology

As sociology has become more institutionalized, it has become more productive. The institutionalization of training has produced a human product with a mastery of some techniques of research, a modicum of a sociological culture, and an ethos which praises research. The institutionalization of research—the institutional provision of research roles, resources, and facilities; the standardization of research procedures; the general approbation of research within intellectual institutions and the larger society—contributes to an increased volume of research. This is true in the United States, above all, but it is also true of countries where the contemporary type of research is a much newer growth, whether it is either a new implantation or a renewed practice of an older tradition.

This change has taken place under the auspices of "science." So-

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ciologists increasingly believe that their work belongs within the family of science: they wish to be scientific. The old discussions regarding the differences between the natural and social sciences have evaporated with the arguments which demonstrated that sociology had a rightful place in "the classification of the sciences." The discussion as to whether sociology is a natural science is cold mutton. Part of the scientific self-conception of sociology is expressed in its preferred form of publication, the journal article. With the multiplication of the literature, the size of the subcommunity of sociologists is becoming large in almost every subfield of sociology; a sociologist can now spend his whole life or most of his sociological career as a specialist on military organization, the aged, the mass media, urbanization, the police, or narcotics addicts. Many of the specialized fields acquire and establish an identity of their own with their own elites and counterelites, their own culture, sometimes their own journals. The literature of the other specialized fields becomes more and more remote from them. As sociologists become "professionals," as so many proudly aver, specialization is accepted sometimes with the stoicism counseled in *Wissenschaft als Beruf*, sometimes with an air of self-congratulation. The new professionals of sociology are a tough-minded lot. They shun armchairs, they call questionnaires "instruments," they speak of "research technology," they are conversant with the language of computers, they are condescending toward "grand theory," negotiations for grants for research projects are frequently on their minds and lips. They are building a science, and as a science under present-day conditions, it must be specialized and it must be cumulative.

But is it cumulative? Many years ago I wrote about the discontinuity of sociological development; I had in mind among other things the flightiness of sociologists who take up a subject, cultivate it for a time, and then drop it before the subject has been brought to intellectual fruition. I had in mind, as instances, human ecology and primary group studies. I also had in mind not only the dropping of subjects or themes before their fulfillment but also the more specific unconnectedness of exact investigations of the same variables. This latter type of discontinuity between specialized investigations of what are ostensibly the same variables persists relatively undiminished, despite the greatly increased "scientificity" of sociology.

Specialization has its limits. The limits are not those given by the sterility of an uninhibited narrowing of the focus of attention—of "knowing more and more about less and less." They are limits engendered by the same institutionalization which has made specialization possible. There forms within each country (and increasingly between countries) an intellectual community, the members of which

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read each other's works, some of them even before publication. They read more or less the same body of literature: many of them are in contact with each other not only through reading each other's writings but also though correspondence and through personal meetings at conferences and on visits. There are, of course, disagreements within these specialized communities regarding techniques, findings, and interpretations, but they try to iron out differences and to avoid them in the future through the standardization of categories and techniques. Nonetheless, these specialized fields do not wholly isolate their members from those of other specialized fields.[37] No field fails to impinge on another equally specialized field. Thus, for example, a sociologist studying the aged cannot avoid the necessity for studying the literature on the family, on urbanization and demography, on geriatric medicine and medical sociology, and so on. As a result, the consensus on techniques of research, findings, and lines of interpretation extends beyond the boundaries of any community of specialists into adjacent communities of specialists. In this way a series of overlaps links the entire profession of sociology. Specialization in sociology has not yet progressed to the point, nor is it likely to do so in the foreseeable future, where one field cuts itself off entirely from its intellectual neighbors or from the sociological family as a whole.

There are, furthermore, limits to specialization imposed by the dominance within the institutional system of the tradition which is embodied in the essay, and less frequently in the treatise. After all, the institutionalization has occurred in universities, and in universities abstract ideas have a pride of place. If a postgraduate student has done sociology as an undergraduate, he has acquired a culture which is common to sociologists; even if he has done a different subject as an undergraduate and begins his sociological studies in a situation committed to specialized research, he must nonetheless absorb a certain amount of the common culture which is made up largely of the dominant tradition and some of the subsidiary traditions.

Sociological training is much less specialized than sociological research. A student must cover a much wider body of literature than that directly pertinent to the field in which he will do his research. Sociology now being an academic subject, the leading research workers tend also to be teachers of sociology; this means that they have often to teach subjects which are somewhat outside their specialized field of research. This, too, has a unifying effect, although it does not create or maintain a completely common sociological culture. The fact that sociology is no longer the work of amateurs, but is taught and largely practiced within universities in which "theory" has prestige, means that the prestige-conferring "classics" are taught in practically all

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universities for better or for worse. There is a *tronc commun* from which sociologists derive some of their intellectual dignity. Durkheim, Weber, Mannheim, Parsons, Merton are repeatedly quoted and invoked over a very wide range of specialized fields of research.

These famous sociologists are so generally drawn in, not just because those who draw them wish to avoid the appearance of being no better than intellectual hewers of wood and drawers of water. They are drawn in because there is a deeply felt need among sociologists to be part of an ongoing intellectual tradition and because there is a no less deeply felt need to explain one's particular findings by reference to certain major variables, such as solidarity, equality, deference, authority, charisma, bureaucracy. Contemporary sociologists are as much the creators of their tradition as they are its beneficiaries and prisoners.[38]

It might be said that the unifying classics of sociology are important to its contemporary practitioners because sociology is not at present a real science. If sociology were a science, its master-works would have become so assimilated into the flow of sociological work that their accomplishments would have been taken for granted. They remain important because sociological analysis is so much devoted to the elucidation of certain major themes which continue to dominate the attention of sociologists, partly because the great figures drew attention to them, and partly because experience and the concern which they have molded have shown them to be the proper subjects of study.

An effective intellectual tradition is not merely a timeless gallery of works, nor is it merely a sequence in time of works arranged by later scholars. An effective intellectual tradition is a linkage of works through influence exercised over time. This is the kind of tradition which operates in the real sciences and it also operates in sociology. In the real sciences, intellectual influence is usually fairly immediate—there are exceptions—and the works which are constitutive of tradition function through entering into the continuing flow; they lose their identity in so doing. Such tradition-setting works provide a framework of concepts which sets research tasks—and they provide the next stage of research, particular hypotheses to be revised, rejected, or confirmed, and observations which may be accepted as given or which require reinterpretation. In sociology the situation is both similar and different.

It is similar in the sense that major works provide a framework of concepts which set research tasks. It is different in that particular investigations are only loosely and vaguely linked with their guiding and legitimating ideas and are also only very loosely linked with those which have gone just before, although treating presumably the same problem at a more advanced stage. This situation in which the

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"subject—[is] presented [as] a library of the works of deceased giants, to be admired and discussed but most unlikely to be extended save by the rare appearance of new genius,"[39] must be superseded by a more continuous process in which one investigator takes up where his predecessor left off.

It is at just this point that the great difficulty of present-day sociology lies. To take up where one's predecessor left off entails the acceptance of the delineation of the variables studied by the predecessor in exactly the form in which they were put by the predecessor. The point of leaving off in sociology is difficult to locate because the vague interpretations of data always exceed the data. The categories involved in interpretation, that is, the theoretical construction, are always broader and vaguer than the categories in which the data were collected or the observations made. The latter vary too much from one investigator to another, even though the investigators believe themselves to be working on the same problem. For good reasons or for poor, each subsequent investigator "improves" on his predecessor's working delineation of the relevant variables. What in fact happens is that a slightly different variable replaces the one previously studied; they are both thought to represent the same thing when in fact they do not. As a result, the cumulative and progressive development of sociological knowledge is rendered more difficult. The source of the fault lies in the "great ideas" of sociology. They obviously refer to very important things—otherwise they would not hold the attention of sociologists as they have for so many decades—but they are extremely ambiguous and they defy authoritative clarification.

The unity which transcends specialization in sociology rests on this common devotion to a relatively small number of "key words." That unity is very expensively purchased. The key words and the ideas which they evoke have become inexpungibly enmeshed in the sociological tradition, so much so that they can never be merely an honorific decoration. They have become constitutive of sociological analysis. But the fact remains that they weigh like an Alp on the sociological mind. Theory is recognized as such by the presence of those alpine key words in all their misty and simple grandeur. This is not good enough. Sociology needs a much more differentiated set of categories, a much more differentiated set of names for distinguishable things. It must name many more things and name them in agreed and recognizable ways. The "slippage" between "concepts" and "indicators" must be reduced by increasing and refining the variety of "concepts."

This situation has arisen from peculiar features of the institutionalization of sociology and the flow of its traditions. Sociology arose both in France and in Germany informally, in a form which did not find at the

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time of its formation incorporation into the academic system. (In France the immediate or proximate institutionalization which Durkheim formed around himself was not incorporated into the ongoing university-institutional system.) The two great founders of sociology worked within humanistic faculties and themselves drew on traditions in which the technique of evidence was mainly those current in philosophy and historiography. Unlike the natural sciences in which theory and empirical investigation developed in close and reciprocal connection with each other, the dominant tradition of sociology developed without such a relationship. What Durkheim did on suicide and Weber on the selection and adaptation of industrial workers and on the East Prussian agricultural laborers showed that the founders were aware of the need for exact evidence for their theories; the fact that most of their work did not employ such evidence showed the strength of the traditions from which they had come. The same disproportion occurred in the United States, even though both the central and subsidiary traditions became institutionalized simultaneously. Despite their simultaneous institutionalization, they could not be integrated. At Columbia University, under the inspiration of Lazarsfeld and Merton, from the late forties into the sixties they came closer to integration than elsewhere, but there too the integration was loose and discontinuous. The gap persists and it is difficult to see how it is to be closed. But closed it must be.

References

1. Observation includes second- and third-hand observation through informants, documents, and printed works.

2. The situation was little different in political science. The intellectual sources of present-day academic political science were much more heterogeneous than the forerunners of sociology. Bentham, Constant, James and John Stuart Mill, Tocqueville, Ostrogorski, Bagehot, Bryce, et al. were not engaged professionally in university teaching and research. Hegel at the beginning of the century and T. H. Green, A. L. Lowell, J. W. Burgess, Jellinek, Gaetano Mosca, Woodrow Wilson, and Léon Duguit at the end were university teachers.

3. Spencer's *Descriptive Sociology* played no part whatsoever in his installation as a source of sociology—which in its subsequent form departed very far from his ideas and has only recently showed a partial turn toward them.

4. Because the EPHE was not dominated by professors, it was possible for Durkheim to attract to his circle young men who were preparing to enter other disciplines. This would have been impossible in Germany. There, a professor would not hold commerce with a research student who did not give promise of becoming a disciple, and a student would not dare to risk his career by serving a professor outside his own subject, the subject in which he wished to make his career. French professors were also jealous and students also depended on them for patronage.

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Nonetheless, Durkheim did attract young men who could not make a career in sociology and who would ultimately have to make a career in some other academic discipline. Perhaps this is to be accounted for by the power of Durkheim's intellectual personality and by the fact that in the fields from which Durkheim drew his adherents, there were so few opportunities that even subservience to the incumbent professor was professionally fruitless.

5. Von Wiese's pupils did do a small amount of field work; see, *Das Dorf als Soziales,* Kölner Vierteljahreshefte für Soziologie, Beiheft I (1928). The result was simply the discovery of illustrations of the concepts of *allgemeine Beziehungslehre.* Dr. Willy Gierlichs, a pupil of von Wiese, taught in a police training college. Again this first step in the institutionalization of sociology in an applied form led to nothing because von Wiese chose the most sterile part of Simmel's sociology to try to develop. Neither the conception of formal sociology nor of *Zu- und Auseinander* was a very fertile point of departure, and von Wiese did not have the imaginative powers to draw out of them what little they contained.

6. In the index of the *International Encyclopaedia of the Social Sciences,* there are two references to Hobhouse, fifty-eight to Durkheim, and sixty-four to Weber.

7. Westermarck, whose own work was in the tradition of German *Völkerkunde,* had one pupil who transformed the subject—that was Malinowski and what he did became anthropology.

8. Institutionalization includes incorporation into the academic system, not just within a single university; other universities increase the density of self-consciousness, and they provide *debouchés* for students produced by the training system.

9. Professor Talcott Parsons spent one year at the London School of Economics before going to Heidelberg for a year. Hobhouse, whose seminar he attended, made no intellectual impact on him. All he could say of it years later was, quoting Crane Brinton: "Who now reads Herbert Spencer?" Weber, who had been dead about five years when Parsons came to Heidelberg, made a profound impression of life-long duration. It is obvious that institutionalization is not the only factor which determines intellectual impact. In both Germany and England, in their different ways, sociology was very slightly institutionalized academically. The power of the intellectual message, the pregnancy of the problem, the scope of its ramifications, the persuasiveness of the argument (insight, logic, evidence), and the magnetism of the rhetoric are surely of importance. Yet the fact that Weber had no impact in Germany despite his greatness and his ubiquitous fame shows, I think, that the factor of institutionalization is an independent variable of some importance.

10. The greatest British survey was *The Life and Labour of the People of London,* in seventeen volumes, the culmination of a tradition begun in the *Reports* of the poor law commissioners and the work of several great officials interested in public health, housing, and "sanitary conditions." Practically none of this work was done under academic auspices. The surveys of the middle of the century were done by government officials. Mayhews' *London Labour and the London Poor* was the work of a journalist, and Booth was a wealthy shipping merchant who financed and directed his survey as a private inquiry. Rowntree, too, was a wealthy businessman without university connections.

When in the 1920's a *New Survey of London Life and Labour* was undertaken, it was directed from the London School of Economics, but it had no con-

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nection with the Department of Sociology at the school. Furthermore, it was a transient enterprise. When the survey was finished, the organization through which it had been conducted was disbanded. It might be pointed out here that when the Population Investigation Committee functioned at the London School of Economics and produced important works in demography and social selection, it worked wholly independently of the Department of Sociology. It did not serve to train and recruit professional sociologists, except for Professor David Glass, who was at that time a geographer and came into the Department of Sociology only after the Second World War. The Department of Social Research, which was created after the war, was an entirely separate entity from the Department of Sociology, but it at least represented the institutionalization of social research.

11. As did William Graham Summer at Yale. Summer never went beyond this, and as most of his students were undergraduates who had no interest in pursuing sociology further, Yale did not become an important center of sociology until many decades later.

12. Thomas spent the academic session of 1888-1889 at the Universities of Berlin and Göttingen. This was before he enrolled as a Ph.D student at the University of Chicago, which was, in fact, nonexistent during Thomas' German years.

13. He undoubtedly encountered the ideas of Tönnies about *Gemeinschaft* and *Gesellschaft,* but there is no reference to Tönnies in his dissertation, *Masse und Publikum,* or in his autobiographical reminiscences. He did reproduce a section from Tönnies' *Die Sitte* (1913) in the *Introduction to the Science of Sociology* (Chicago: University of Chicago Press, 1921), pp. 103-105.

14. Norbert Elias, *Uber den Prozess der Zivilisation,* 2 vols. (Basel: Verlag zum Falken, 1938); Hans Gerth, *Die sozialgeschichtliche Lage der burgerlichen Intelligenz in Deutschland um die Wende des 18ten Jahrhunderts;* Wilhelm Carlé, *Weltanschauung und Presse;* Jakob Katz, *Die Judenassimilation im Deutschland.* The latter three were printed only as dissertations.

15. Westermarck's chair was filled only during the Easter term of each year from 1907 to 1930.

16. Walton Hamilton, prior to going to Amherst, where his pupils included Talcott Parsons, was at the University of Texas, away from the pressure of conventional economics. It should, however, be acknowledged that Johns Hopkins University in Baltimore played an early part in this introduction of German historicism into the United States. But Johns Hopkins was *the* new university of the country until Chicago was founded. (Simon N. Patten does not fit into the picture presented above.)

17. Science in the heroic age should be reexamined in the light of the approach taken in this paper. Aside from the genius of its main actors, it owes some of its growth to its informal organization as a society of correspondence. The directly addressed audience of known persons remains an important feature of contemporary science. The publication of books and papers apparently addressed to an almost entirely anonymous audience does not mean that their contents move in all directions randomly and indifferently to their authors' intentions.

18. It is unlikely that his "Agrarverhältnisse im Altertum" and *Romische Agrargerchichte* . . . were unknown to Italian classical historians.

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19. Alexander von Schelting understood Max Weber's ideas with a precision and elaborateness which was unique in Germany. He was not, however, *habilitated* until after the end of the Weimar Republic, when he went into exile and taught briefly at Columbia, before the renaissance created by Merton and Lazarsfeld. Then, after years of poverty in Switzerland and a brief period as an *Extraordinarius* in Zurich, he died without influence and with the sociological part of his study of the Russian intelligentsia unfinished.

20. Weber's methodological views and the Protestant Ethic thesis were the only aspect of his work with which German scholars were concerned in the 1920's. Jaspers and Rickert, who were closest to Weber's outlook, were interested exclusively in his methodological writings, as was von Schelting before his exile. The Protestant Ethic thesis was seriously discussed by historians but little of this flowed into sociology.

    It should also be said that the fading of the imprint of Weber's ideas was fostered not just by the absence of any sustaining intellectual-institutional structure around Mannheim, but also by Mannheim's hidden and ambivalent struggle to establish a distinctive line of his own different from Scheler's, different from Marx's, and different from that of German idealism.

21. The diffusion of Max Weber's ideas in America also owed something to Frank Knight who was the first translator of any work of Weber into English. His translation of *Wirtschaftgeschichte* bore the English title, *General Economic History* (1927). Knight was an extraordinarily acute and subtle economic theorist who saw the limits of neoclassical theory and who was also aware of the fundamental intellectual weakness of "institutional economics." In the middle 1930's, Knight conducted a seminar on the first chapters of *Wirtschaft und Gesellschaft*, in which I participated, where we studied the text line by line. Melchior Palyi was at that time on the staff at Chicago; he had been Weber's assistant at Munich and after Weber's death had helped to establish in the text of *Wirtschaftgeschichte* with Professor Hellmann and of *Wirtschaft und Gesellschaft* with Marianne Weber. He encouraged my Weber studies, although I had first discovered Weber through Tawney, Troeltsch, and Burkharin.

22. The subsequent career of the Weberian sociology through Parsons' role in the Department of Social Relations at Harvard is dealt with below.

23. The one form in which Durkheimian tradition was institutionalized was in the textbooks of civil morals in the *lycées*. But rather than recruit sociologists, it alienated persons who might have become sociologists. Raymond Aron was one of those who was alienated from Durkheim's tradition by this mode of presentation. He was recovered only by encountering Max Weber when he went to Germany at the beginning of the 1930's.

24. Durkheim exerted through Radcliffe-Brown a marked influence on British social anthropology, but until very recently the social anthropology deriving from Radcliffe-Brown and Malinowski had very little connection with British sociology.

25. It is interesting that at a not very distant time, Max Weber had studied the German side of the same problem—the impact of the immigration of Polish agricultural laborers into the East Elban landed estates—while Thomas was studying the consequences for the Polish immigrants of their movement into the United States. There is no evidence that they were aware of each other's work.

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26. Thomas' *Source Book for Social Origins* contains nothing by Durkheim and there are only two references to articles by Durkheim in a bibliography of forty-two pages.

27. I knew him well in the period just after his retirement from the University of Chicago and often used to talk with him, or rather listen to him, but he never mentioned Durkheim or Weber, even though he knew I was more or less conversant with European sociology. I attended his last lectures at the University of Chicago in the spring of 1934, and, although Bagehot, Tarde, Sighele, and all sorts of books including Theodore Geiger's *Die Masse und ihre Aktion* were mentioned, Durkheim was passed over without a word. There were, however, two excerpts from Durkheim in *Introduction to the Science of Sociology*.

28. I am also not certain about how Elton Mayo came to be interested in Durkheim. Perhaps via Radcliffe-Brown or Lloyd Warner.

29. Although Durkheim disregarded foreign writers who called themselves sociologists, just as he disregarded French *soi-disant* sociologists, he did make much use of ethnographic work from Germany, Great Britain, and to some extent, the United States. Spencer and Gillen on Australia was the factual foundation of *Les Formes elementaires de la religion.*

30. I refer here to the awkwardness of present "elite studies." Attempts to legitimate such studies are sometimes made by associating them with the study of power and authority—very unconvincingly—or with the "circulation of elites," which likewise has not found thus far a comfortable home in contemporary sociology.

31. The variant of the Darwinian tradition elaborated by Ratzenhofer and Gumplowicz had an enduring after-life via Albion Small, W. I. Thomas, and Robert Park. The affinity of this conception of conflict to competition in the animal and plant worlds had a powerful effect on Chicago sociology. It also had a pronounced influence on Arthur Bentley and through him on American political sociology.

32. A characteristic allegation of this sort is combined in the essay by Professor Ralf Dahrendorf, "Out of Utopia," *American Journal of Sociology,* 61 (1958).

33. It must be remembered that what is institutionalized is an intellectual process and that an account of the process is not exhausted by an account of the institutional setting in which it occurs. There is the sheer power of intelligence in confrontation with problems. The problems can be perceived through a number of mechanisms. They can be contained in the intellectual works which a searching, questing intelligence has discovered—either by its own curiosity or by moving along paths which it has made for itself in only very lightly chartered territory. Intellectual works themselves are linked with other intellectual works contained in them by references to other books and authors. Even without the aid of the contemporary institutional elaboration of teaching, bibliographical services, libraries, and learned societies, a seeking intelligence with the sensitivity of an Indian tracker could find like-minded persons across the stretches of a relatively unorganized Europe. Nonetheless, the task was more difficult before institutionalization through universities and learned journals.

34. During the depression years of the 1930's, interesting investigations were made into the effects of unemployment on individual morale, family life, and so forth.

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Such studies were dropped from the agenda of sociological research when nearly full employment replaced massive unemployment.

35. The collection of essays is different from the "reader," which in sociology is a more "scientific" type of textbook than the conventional compendium of discursive expositions of concepts and illustrative data. The "reader" as a textbook is usually a collection of research reports which have appeared as articles in journals. The editors, unlike the authors of textbooks in scientific subjects, do not consolidate existing knowledge as it bears on particular problems; they simply present the sources—journal articles—from which in scientific textbooks an interpretative consolidation is made.

36. The tendency, now widespread among social scientists, to distribute their writings in mimeographed, dittoed, or xeroxed form of publication is not an effort to inform the rest of the social science community of results achieved and verified, but rather the opposite. The distributed paper is a preliminary version, and the distribution is an effort to elicit criticisms; it is an acknowledgment of the tentative and uncertain status of what is presented in the papers circulated.

37. One of the reasons for the sterility of much of experimental social psychology is that it has become an isolated "specialism" which is content to remain within the limits fixed by the identities of men and pigeons.

38. One of the difficulties is that we cannot imagine anything beyond variations on the themes set by the great figures of nineteenth- and twentieth-century sociology. The fact that the conception of "post-industrial society" is an amalgam of what St. Simon, Comte, Tocqueville, and Weber furnished to our imaginations is evidence that we are confined to an ambiguously defined circle which is more impermeable than it ought to be.

39. A. H. Halsey, *Encounter* (April 1970).

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TALCOTT PARSONS

On Building Social System Theory: A Personal History

THE EDITORS have urged contributors to be autobiographical and to write informally. In this vein, I may start by saying that the main subject matter will be the evolution of a contribution to the generalized theoretical analysis of the phenomena of human action, with special concern for its social aspects—that is, the theory of the social system. This has entailed combining elements from a variety of sources, combinations that are not frequently tapped by persons more inclined to disciplinary specialization.

I was perhaps predisposed in this direction by a highly unorthodox education. As an undergraduate at Amherst, influenced by an older brother who had gone into medicine, I had intended to concentrate in biology with a view either to graduate work in that field or to a medical career. But in 1923, my junior year, I was converted to social science under the influence especially of the unorthodox "institutional economist," Walton Hamilton. Then all concentration plans were disorganized by the dismissal, at the end of my junior year, of Alexander Meiklejohn as president of the college. None of the teachers whose courses I had elected was on hand the next fall. I made do with more courses in biology, some in philosophy, including one on Kant's *Critique of Pure Reason*, and some in English literature.

From the beginning I assumed I would pursue graduate study. But although sociology in a broad and vague sense attracted me, the regular American graduate programs, though I knew relatively little about them, did not. When an uncle offered to finance a year of study abroad, I elected to go to the London School of Economics. There the names of L. T. Hobhouse, R. H. Tawney, and Harold Laski particularly attracted me. Only after arriving did I discover the man who proved the most important to me intellectually—Bronislaw Malinowski, the social anthropologist.

I was not a degree candidate at London and my plans were unformed, so I was ripe to seize the offer of an exchange fellowship to Germany, for which I was recommended by Otto Manthey-Zorn, with whom

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I had had a seminar in German philosophy at Amherst and who also had much to do with my appointment, a year later, as instructor in Economics at Amherst. I was assigned to Heidelberg with no personal voice in the matter, but this located me where the influence of Max Weber, who had died five years before, was strongest. It is significant that I do not remember having heard his name either at Amherst or in London.

Weber's work, especially *The Protestant Ethic and the Spirit of Capitalism* (which a few years later I translated into English[1]), immediately made a strong impression on me. On going to Heidelberg, I had had no intention of taking a degree, but I subsequently learned that this could be accomplished with only three semesters' credit, oral examinations, and a dissertation. I decided to write a dissertation under the direction of Edgar Salin (later of Basel, Switzerland) on "The Concept of Capitalism in Recent German Literature." I began with a discussion of Karl Marx, said something about some less central figures such as Lujo Brentano, and then concentrated on Werner Sombart (author of the huge work *Der moderne Kapitalismus*[2]) and Max Weber. This work crystallized two primary foci of my future intellectual interests: first, the nature of capitalism as a socioeconomic system and, second, the work of Weber as a social theorist.

During the year of teaching at Amherst, which allowed time for a good deal of work on my dissertation, it gradually became clear to me that I wanted to go thoroughly into the relations between economic and sociological theory. I became particularly indebted to discussions with Richard Meriam, who had come to Amherst as chairman of the Department of Economics after my graduation. Meriam persuaded me that, although economic theory was one of my examination subjects at Heidelberg, I needed to know much more about it, and I decided to make acquiring this knowledge my next step. Although a German Dr. Phil. was not the equivalent of a good American Ph.D., I decided not to become a candidate for the latter. Meriam recommended that I go to Harvard and arranged an instructorship appointment for me there for the fall of 1927.

Allyn Young, perhaps the most important man for my interests, had just gone to England, but I was fortunate in having contact with Harvard economists F. W. Taussig, T. N. Carver, W. Z. Ripley, and Joseph Schumpeter (who had a visiting appointment, though later he came permanently to Harvard). Edwin F. Gay, the economic historian, was thoroughly familiar with the German background of my Heidelberg training and sympathetic to my interests.

Meriam was quite right in maintaining that the knowledge of economic theory I could acquire at Harvard was far superior to that I had

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learned at Heidelberg. It gradually became clear to me that economic theory should be conceived as standing within some sort of theoretical matrix in which sociological theory also was included. I tried my hand at statements of this idea in some articles that Taussig kindly published in the *Quarterly Journal of Economics*,[3] of which he was then editor. Most important, however, was a decision to explore this theme in the work of Alfred Marshall, who at that time was the dominant influence in "orthodox" or "neoclassical" economic theory, with a view to extracting Marshall's "sociology" and its articulation with his strictly economic theory. The results, published in 1931-1932,[4] crystallized the first stage of a theoretical orientation that seemed to me to go beyond the levels attained by my teachers in articulating the theoretical structures of the two disciplines.

Contact with Schumpeter was particularly helpful in laying the groundwork for this development because, on the problem of the scope of economic theory, he was a strict constructionist in contrast to Marshall's refusal to draw any sharp boundaries. Knowledge of Vilfredo Pareto, gained both on my own and through contact with L. J. Henderson, was also especially important because Pareto had been an eminent economic theorist, a good deal in the same tradition as Schumpeter, but at the same time he had attempted the formulation of a more comprehensive system of sociological theory which, in his view, included a rather strictly defined economic theory.[5] Thus both Schumpeter and Pareto served as critical reference points from which to attempt to distinguish between the economic and the sociological components of Marshall's thought.

From this starting point there gradually evolved the project of including not only Marshall and Pareto—on whom I had written a monograph-length analytical paper shortly after completing the Marshall study[6]—but also Weber and Durkheim, in a comprehensive study of this group of "recent European writers." Weber's general ideas on the nature of modern capitalism, which had been the principal focus of my dissertation, and more specifically his conception of the role of the ethic of ascetic Protestantism, provided sufficient ground for hope that a Marshall-Pareto-Weber "convergence" could be worked out.

Increasingly, it seemed to be desirable to include Durkheim in the picture, but this presented greater difficulties. Of the four, Durkheim had had by far the least to do with economics as a technical discipline. Moreover, I never had a teacher on Durkheim in the same sense that my Heidelberg teachers were such on Weber, Taussig and Schumpeter were on Marshall, and Schumpeter and Henderson were on Pareto. Furthermore the exposure to Durkheim I had been given, especially by Ginsberg and Malinowski in London, was not only not very helpful, but also positively misleading: much unlearning of what was not true about Durk-

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heim became necessary. The clue, however, was available. It lay in Durkheim's first major work, *De la division du travail social* (1893),[7] which interestingly was seldom mentioned in the English language secondary literature of that period. Careful study of this book showed that its analysis could indeed be very directly articulated with Weber's analysis of capitalism, and that in turn with Marshall's conception of free enterprise. The theory as such then articulated with the sociological, rather than the strictly economic, components of the work of Pareto and Weber and, more indirectly, Marshall. The key conceptual complex concerned the institutional framework of property and especially contract as distinguished from the "dynamics" of economic activity as such, and as constituting, for its understanding in the theoretical sense, a field for sociological rather than economic investigation.

A First Major Synthesis

The outcome of this complex series of studies was *The Structure of Social Action,* published in 1937 but completed in first draft—though substantially revised afterward—nearly two years before.[8] The book was meant as a study of the writers' ideas about the modern socioeconomic order, capitalism, free enterprise, and so forth, and, at the same time, of the theoretical framework in terms of which these ideas and interpretations had been formulated. In this respect the most immediate interpretive thesis was that the four—and they did not stand alone—had *converged* on what was essentially a single conceptual scheme. In the intellectual milieu of the time this was by no means simple common sense, but as it unfolded it surprised even me a great deal.[9]

To arrive at this conclusion required three resources in particular. One, of course, was careful critical study of the rather large body of relevant texts, as well as secondary literature, though most of the latter was worse than useless. The second was the development of a theoretical scheme that would be adequate to the interpretation of these materials. The third, finally, in a sense lay back of that. It included a kind of philosophy of science orientation about which a few things ought to be said.

All persons aspiring to some degree of sophistication in intellectual fields, since long before the 1920's when these problems became salient to me, had to develop some conception of the nature and conditions of empirical knowledge, and in particular of the nature and role of theory in that knowledge. I was introduced to such problems partly through courses in empirical science, especially biology, and partly through philosophy, including, as I have noted, an intensive course in Kant's *Critique of Pure Reason.*[10] The Heidelberg experience carried me sub-

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stantially farther, particularly in studies of the issues involved with Weber's *Wissenschaftslehre*. Notable among these were, first, the problems centering around the German historical traditions and hence the status of generalized theoretical conceptualization in the social and cultural disciplines, and, second, those of the status of the interpretation of subjective meanings and motive in the analysis of human action, what the Germans called the problem of *Verstehen*.

Returning to this country I found behaviorism so rampant that anyone who believed in the scientific validity of the interpretation of subjective states of mind was often held to be fatuously naïve. Also rampant was what I called "empiricism," namely the idea that scientific knowledge was a total reflection of the "reality out there" and even selection was alleged to be illegitimate.

Weber had insisted on the inevitability and cognitive validity of selection among available factual information. The importance of analytical abstraction was further strongly emphasized by Henderson in his formula: "A fact is a statement about experience in terms of a conceptual scheme."[11] The culmination of this conception came for me in A. N. Whitehead's work, especially his *Science and the Modern World*, including his illuminating discussion of the "fallacy of misplaced concreteness."[12] Through channels such as these I arrived at a conception which I called "analytical realism," which treated the kind of theory I was interested in as inherently abstract but by no means as "fictitious" in the sense of Hans Vaihiger.[13] This seemed to me in particular to fit the treatment of the status of economic theory by Schumpeter and Pareto. I also found various of James B. Conant's writings on the nature of science, especially the role of theory, very helpful.

Closely related to this was the conception of "system." Schumpeter and Whitehead were important in forming the background of this concept, but I think it crystallized above all in the influence of Pareto and Henderson. As Henderson was never tired of pointing out, Pareto's model in this respect was the idea of system as used in the theory of mechanics, but he attempted to apply it both to economics and to sociology. Hence Henderson's statement that perhaps Pareto's most important contribution to sociology was his conception of the "social system," a dictum which I myself took so seriously that I used the phrase as the title of a book some years later.

Henderson's own primary model, which he explicated at some length in *Pareto's General Sociology*, was the physico-chemical system.[14] He related this, however, to biological systems. He was a great admirer of Claude Bernard and wrote a foreword to the English translation of the latter's *Experimental Medicine*.[15] The central idea here was that of the "internal environment" and its stability. This connected closely with

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the idea of W. B. Cannon of homeostatic stabilization of physiological processes and also with the residues of my own exposure to biology.[16]

Thus a certain foundation for the transition from the concept of system as used in mechanics and the physico-chemical system as explicated by Henderson to that of the special character of "living systems" was laid in these early years. This was essential to the later phase of my thinking which is usually referred to as "structural-functional" theory and culminated in my book *The Social System.*[17] Further steps were greatly influenced by a continuing Conference on System Theory held from about 1952 to 1957 under the chairmanship of Dr. Roy Grinker in Chicago. Among the several participants whose ideas were important to me, the social insect biologist Alfred Emerson stands out. What he had to say, including some of his writings, strongly reinforced my predilection for the homeostatic point of view of Cannon. He spoke, however, in such a way as strongly to predispose me, and I think others, in favor of the then just emerging conceptions of cybernetic control, not only in living systems but also in many other kinds of systems. This later became a dominant theme in my thinking.

Finally, Emerson put forth what turned out for me to be an especially fruitful conception which did much to seal my conviction of the fundamental continuity between the living systems of the organic world and those of the human sociocultural world.[18] This was the conception of the functional equivalence of the gene and, as he put it, the "symbol." We can perhaps restate these as the genetic constitution of species and organism and the cultural heritage of social systems. In more recent years this perspective has come to be of fundamental theoretical importance to me.

Personal and Professional Concerns

The Structure of Social Action marked a major turning point in my professional career. Its major accomplishment, the demonstration of the convergence among the four authors with which it dealt, was accompanied by a clarification and development of my own thought about the problems of the state of Western society with which the authors were concerned. The state of Western society which might be designated as either capitalism or free enterprise—and on the political side as democracy—was clearly then in some kind of state of crisis. The Russian Revolution and the emergence of the first socialist state as controlled by the Communist party had been crucial to my thinking since undergraduate days. The Fascist movements affected friendships in Germany. Less than two years after the publication of the book the Second World War was to begin, and, finally, came the Great Depression with its ramifications throughout the world.

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My personal concerns involved a growing family[19]—three children born from 1930 to 1936—and difficulties of status at the professional level. Though the situation was not strictly comparable to what it would be now, it was even then anomalous that I remained at the instructor level for nine years, the first four in the Department of Economics, the last five in the newly constituted Department of Sociology. I had the misfortune of serving under unsympathetic chairmen—in economics the late H. H. Burbank, in sociology P. A. Sorokin. My promotion to an assistant professorship occurred in 1936 and was pushed not by Sorokin but, notably, by E. F. Gay, E. B. Wilson, and Henderson, all of whom were "outside members" of the Department of Sociology. The first draft of *The Structure of Social Action* was in existence by then, and known to all the principals.

Even with the assistant professorship, however, I was by no means certain I wanted to stay at Harvard. In the (to me) critical year of 1937 I received a very good outside offer. Since Gay had retired the previous year and gone to California, I went to Henderson—not Sorokin. In those days before the ad hoc committee system, Henderson took the matter directly to President Conant who, with Sorokin's consent, to be sure, offered to advance me immediately to the then extant "second term" assistant professorship, with a definite promise of permanency as associate professor two years later. On those terms I decided to stay at Harvard.

On the intellectual side I have noted that I had very good relations with Taussig, Schumpeter, and Gay. The above crisis occurred shortly after the completion of an extraordinary relationship with Henderson. I had known him through his Pareto seminar and in other respects before the manuscript of my book was referred to him for critical comment in connection with my appointment status. Instead of the usual limited response, he got in touch with me (stimulated I think mainly by my discussion of Pareto) and started a long series of personal sessions at his house, something like two hours, twice a week for nearly three months. In these sessions he went through the manuscript with me paragraph by paragraph, mainly the sections dealing with Pareto and Durkheim; he passed over Marshall rather quickly and did not enter into my treatment of Weber at all.

This was an extraordinary experience, both personally and intellectually. Those who knew him will remember that Henderson was a formidable person, who could be dogmatic, both on political matters, in which he was a pronounced conservative,[20] and on many intellectual matters, as exemplified by his unfairness—as I judged it—to all but one or two sociologists. He had, however, an immense knowledge of science, especially on the level of the philosophy of science and the nature of

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theory, and if one stood up to him and did not allow oneself to be overridden, he was an exceedingly insightful critic and very helpful with precisely *my* intellectual problems. I benefitted enormously from this special contact, and took about a year to complete the revisions which the discussions convinced me should be undertaken.[21]

In these early Harvard years, I also had important experiences with age-peers and, in course of time, students. A group of junior faculty members that met fairly regularly included Edward S. Mason, Seymour Harris, Edward Chamberlin, and for a time Karl Bigelow in economics, Carl J. Friedrich in government, and Crane Brinton in history. With the move to sociology, which brought me closer to psychology and anthropology, I began to get to know Gordon Allport, who had recently returned to Harvard from Dartmouth, and Henry Murray. In anthropology, there were two particularly significant contemporaries. The first was W. Lloyd Warner, brought to Harvard mainly by Elton Mayo, who, under Henderson, directed the Western Electric Study, to make a community study that eventually became the well-known "Yankee City Series." When Warner left Harvard to go to Chicago he was replaced by Clyde Kluckhohn, a young social anthropologist who was quite independent of the Henderson group but associated with them. He became a close friend of Murray, and with Allport, Murray, and Kluckhohn the nucleus of the eventual promoters of the Social Relations experiment was present.

Also by the mid-thirties significant relations with graduate students, a few of whom received teaching appointments, began. The most important single one was with Robert Merton, who was in the first contingent of graduate students in sociology, but after him came Kingsley Davis, John Riley, and—not as a student—Mathilda Riley, Robin Williams, Edward C. Devereux, Logan Wilson, Wilbert Moore, Florence Kluckhohn, and Bernard Barber. An informal discussion group on problems of sociological theory composed largely of these people met in the evenings in my study in Adams House, while I was still an instructor.

Theoretical Interests after The Structure of Social Action

Completion of *The Structure of Social Action* constituted a gratifying accomplishment, though at that time I was not aware how large a reputation it would achieve.[22] The theoretical framework which had enabled me to demonstrate the thesis of convergence clearly had the potential of further use and development, but there were several alternatives of what to attempt next with its help. In the Bellagio conference, where this essay was first presented, there was considerable discussion of why I did not identify with economics as a discipline. By the time

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The Structure of Social Action neared completion that decision had already been made on one level by my transfer from economics to the new Department of Sociology. In spite of the friendliness of Taussig, Gay, and Schumpeter, I am quite sure I could not have counted on a future in economics at Harvard. But basically I did not want to do so, and in retrospect the focus of the reasons lay in my permeation with Weber's thinking, then Durkheim's: Freud was yet to come. Though I wanted to keep my contact with economic theory, and in fact have done so in various ways, I saw clearly that I did not want to be primarily an economist, any more than Weber turned out to be.

There was one interesting episode which might, at a relatively late time, have turned me at least farther in the direction of economics. After my formal transfer to sociology, Schumpeter organized a small discussion group with younger people, mostly graduate students, on problems of the nature of rationality. After a few meetings he proposed to me that the group should aim at producing a volume, of which he and I should be at least coeditors, if not coauthors. Though not specifically rejecting the proposal, at least immediately, I remember having reacted rather coolly, and in fact I let it die. I am not wholly clear about my motives, but I think they had to do with the feeling that I needed a relatively complete formal break with economics.[23]

The Professions and the Two Aspects of the Rationality Problem

The actual choice I made was to undertake a study of some aspect of the professions as social phenomena. This interest grew logically out of the combination of my concern with the nature of modern industrial society and the conceptual framework in which I had approached it. It was empirically nearly obvious that the "learned professions" had come to occupy a salient position in modern society, whereas in the ideological statement of the alternatives, capitalism versus socialism, they did not figure at all. Indeed, what is now habitually called the "private, nonprofit" sector of organization and activity which is occupationally organized, as distinct, for example, from kinship, did not figure ideologically. In retrospect it can be said that both ideological positions stated versions of the "rational pursuit of self-interest"—the capitalist version, grounded in utilitarian thought, the interest of the individual in the satisfaction of his wants, the socialist version, the interest of the collectivity (on lines deriving from Hobbes and Austin) in maximization of satisfaction of the public interest.

Within this field I chose to study some aspects of medical practice. This choice had, I think, good technical grounds, but it also goes back to personal motives. Certainly I could say that my previous renuncia-

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tion of biological-medical interests played a part: the role of a sociological student of medical practice was an important way of satisfying both motives. The Henderson-Mayo group, however, also had a bearing. Henderson was himself medically trained, though he never practiced, and his early Harvard appointments were in the Medical School. He had combined his medical and sociological interests in a famous paper, "Physician and Patient as a Social System,"[24] which stated an approach highly congenial to me; hence it was natural that I should consult both Henderson and Mayo—but also W. B. Cannon about my plan of work. All three strongly reinforced my own feeling of the probable fruitfulness of such a study. I proposed, in addition to canvassing the literature, to approach it through the methods of participant observation and interview. The semi-public character of medical practice in the modern hospital made a good deal of the former possible, such as—equipped with a white coat and the (albeit nonmedical) legitimate title of doctor—making ward rounds, observing operations, going on the home visit service of the Tufts medical center and the like. (Perhaps, with current concern over ethical aspects of research, this mild deception of patients would now be regarded as unethical.) The other source of data was a series of interviews with a rather large number of physicians, chosen to represent different types of practice.

One other aspect of the situation involved in this choice turned out to be of primary significance. This was a time in which ideas about psychosomatic relations were beginning to take hold among the intellectual elite of medicine—typified perhaps by internists at the Massachusetts General Hospital, where I spent a good deal of time. In the background was psychoanalysis and the fact that the professor of psychiatry at Massachusetts General Hospital, Stanley Cobb, had recently been the principal founder of the Psychoanalytic Institute in Boston. The Henderson-Mayo group had also been very much concerned with this and related movements of thought—they were devotees of Pierre Janet, but also of Jean Piaget.

For me the decisive event was a talk with Elton Mayo about my interests in medical practice, in which he asked point blank how well I knew the work of Freud. My reply had to be, only very fragmentarily. He then earnestly advised me to read Freud seriously and comprehensively. This was fortunately a time when I had a good deal of free time, thanks to an assistant professor's term leave, and I followed his advice. It was too late to build the implications of Freud's ideas into *The Structure of Social Action*, but this proved to be one of the few crucial intellectual experiences of my life. This, of course, prepared the way for formal psychoanalytic training—at the level permitted—about a decade later.

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I used the economic paradigm of the "rational pursuit of self-interest" as the major point of reference, but in this case negatively, to throw light on the *differences* between the classical economic model of market orientation and the professional case with which I was concerned. Major differences were not far to seek. In the individual practitioner they were, first, fee-for-service relation to patients' situations—the so-called "sliding scale" or higher charges for well-to-do patients and lower or remission of charges for less well-to-do patients. And, second, the objection to "shopping around"—namely patients testing out various physicians with respect to their judgment of the value of the proferred medical service, financially and otherwise. In later years I was to modify substantially this close assimilation—with all the differences—of the professional relation to the ideal type commercial one.

The most important theoretical implications concerned the problem of the nature of rationality, the very question with which I had been involved, not only in my own work, but also in my work with Schumpeter. What opened up was a distinction, not only between economic and noneconomic aspects of rationality, but also, within the latter category, between two different modes or directions of considering the rationality problem. The first concerned a very old problem, even for me, namely the relation between rational (mainly scientific) knowledge and action in the sense of "application." Medicine, especially at the time I was studying it, was a kind of prototype of the possibilities of generating potentially useful knowledge and applying it to the solution of critical human problems. What was called "scientific medicine" then enjoyed a sort of heyday, and its significance was strongly impressed upon me by my brother, who had been trained at Johns Hopkins. There were, of course, very definite connections between this aspect of medicine and the more general setting of the rationality problem in *The Structure of Social Action*, notably in Pareto's concept of "logical action."

The concern with psychosomatic problems, and eventually those of mental illness, noted above, raised a different set of questions. These concerned the significance of the scientific modes of rational investigation and analysis for the understanding, and in some eventual sense control, of the non- and irrational factors in the determination of human action, in the first instance individual, but clearly also social. Concern with these issues permeated the thought of all of my authors except Marshall, but intensive contact with Freud rounded out the pattern and gave new dimensions to it, notably concerning the relevance of nonrational factors and mechanisms in the more intimate microsocial processes of interaction. I suspect that concern with this complex of problems had a good deal to do with my coolness toward Schumpeter's overture, flattering as that was for a young and still insecure scholar.

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In this type of interest I was clearly influenced by my growing association with such colleagues as Clyde Kluckhohn and Henry Murray; Gordon Allport, by contrast, was a psychologist of pronouncedly rationalistic bent. At any rate, *both* facets of the "rationality complex" happened to be combined in my study of medical practice and both stood in sharp contrast to my earlier concentration on the economic-political aspects of rationality. In the background stood concern for the status of religion in any general analysis of social action—a concern which I had come by honestly as a result of my family background and which was brought into focus by Weber's analysis of the Protestant Ethic and his more general studies on the comparative sociology of religion.

As I see it now, these three (or, including religion, four) foci of the "rationality problem" have been nearly dominant in the structuring of my theoretical interests since this crossroads situation of the late 1930's. The first main swing was away from the economic-political complex toward the socio-psychological, that is, the problem of nonrationality seen more from the perspective of Freud than that of Weber or Pareto, different as these two were from each other. In the course of this move I was fully conscious of the importance of "cognitive rationality" in particular as constituting the cultural basis of the scientific component of medicine. Intensive concern with this, however, was to await my coming around to concern with the anchorage of the professions in the system of higher education and research, a concern that has become central to me in recent years. In a certain sense it is a case of the "return of the repressed."

In the context of the social-emotional aspect of medical practice, I came to analyze some of the phenomena which were then commonly called the "art of medicine" (as contrasted with the "science") in terms of Freud's conception of the relation between analyst and analysand, notably the phenomenon of transference, which I regard as one of Freud's great discoveries. Freud clearly had not invented the physician-patient relationship, which in Western tradition goes back at least to Hippocrates (vide Henderson), but he had accepted it as his primary social framework for psychoanalytic practice and immensely deepened the basis of its understanding. It became clear that the psychoanalytic relation presented the extreme, and, hence, in a kind of limiting sense paradigmatic, example of this relationship, and that the vast and vague range of psychosomatic relationships fitted into this. There was, of course, a basic link between the two primary aspects of the rationality problem of interest here, namely via the aspiration of psychoanalysis to scientific status, which in my opinion in spite of much controversy has been broadly validated and is perhaps in the context of application best symbolized for the process of therapy by Freud's aphorism "where Id was, there shall Ego be."

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It was mainly by this path that I came to think of illness as in some sense a form of social "deviance" and of therapy as belonging in a very broad band of types of "social control"—a view for which I have paid dearly in the accusation of being an agent of Establishment interest in maintenance of the status quo. There is, nevertheless, a certain element of truth in this position, truth which I think to be basically independent of the particular form of social order. The important theoretical point is the shift from considering the application of medical science as a bio-physical technology only, to considering it as a field of social interaction as well. Put in more technical terms which were crystallized later, the traditional view of medical practice conceived it as a relation between cultural systems (scientific knowledge) and organisms, with social agents only implementing the obvious implications of knowledge, whereas the other perspective involved treating medical relationships, at least in part, as cases of the subtle interplay between unconscious motives at the personality level and particularities of the structure of social systems. There are further sociological correlates of the difference which involve a duality of levels—in the medical case between physician as competent agent of social control and patient as recipient of these important services—which cannot be gone into in detail here. This turns out, however, to be a major focus of social structure which does not have a place either in the predominantly economic or the political models (capitalism and socialism).

From Medical Practice to the Theory of Socialization

In a period of my career so much under the influence of Freud, it was perhaps natural to shift interest from the analysis of the social situation of psychoanalysis, and more generally of medical practice, to that of the origins of the problems which analyst and analysand confronted. These clearly were problems in the first instance in the personalities of analysands—the bases of "countertransference" were analyzed somewhat later—and for the sociologist led to the consideration of conditions of child development in the family as a social system. Freud himself had increasingly laid stress on "object relations" but could scarcely be said to have developed an adequate sociology of the family. In this connection a major conception on which there had been impressive convergence came to play a central role. In my reading of Freud, I gradually realized the importance of what I and others came to call the phenomenon of "internalization" (Freud's own term was "introjection")—both of sociocultural norms and of the personalities of the others with whom an individual has been interacting, above all a "socializing agent" (the latter case sometimes being called "identification").

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This idea first emerged clearly in Freud's thought in the conception of the superego, though one may say it was present from an early stage, especially in the conception of transference (for example, the treatment of the analyst as if he were the analysand's father). Freud came to consider moral standards, particularly as implemented by the father, integral parts of the child's personality, through some phases of the learning process. Gradually the scope of this aspect of Freud's theory of "object relations" widened in the course of his later work to include not only the superego, but also the ego and even the id.[25] At about the same time, it became clear to me that a very similar conception, developed from a quite different point of view, was essential to Durkheim, especially in his conception of social control through moral authority. The idea was also at least implicit in Weber's treatment of the role of religious values in determining behavior, and appeared with great clarity in the work of a group of American social psychologists, notably G. H. Mead and W. I. Thomas. The conception of the internalization, in successive series, of sets of cultural norms and concrete social objects has become a major axis of the whole theory of socialization, figuring in new forms even in the most recent treatments of the problems of higher education.

Internalization is a feature of the structure of the personality as a system. I have called the parallel phenomenon for social systems institutionalization, especially the shaping of social relationships by components of normative culture, which come to constitute direct structural parts of the social system of reference. Perhaps the preeminent theorist of this line of analysis was Weber, especially in his comparative sociology of religion, but Durkheim made contributions of similar importance. Both these conceptions, furthermore, could make sense only if the primary subsystems of a general system of action were conceived to be *interpenetrating* as well as interdependent. Thus, certain components of the cultural system came to be *at the same time* components of certain social and personality systems. This very central conception, in turn, depended heavily on the conception of the abstractness of the subsystems under consideration. Thus a social system—for example, a "society"—is not a concrete entity, but a way of establishing certain relations among components of "action" that are distinctive relative to the manifold of concrete reality.

Concern with this set of problems of the nonrational was certainly reinforced by the circumstances of the times. Discussions of the problem of German character, in connection with which Erik Erikson first became a salient figure to me, were important at this time.[26] So were personal matters, including my medical brother's premature death (1940) and the aging and eventual deaths of my parents (1943 and 1944).

This set of circumstances seems to me to provide the principal ex-

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planation of a major failure of my career: not to carry through my intention to publish a major monographic study of medical practice. I had, I think, gained immensely from the venture, but had been diverted into more general concerns with the subtler aspects of social control and the genesis of the problems in socialization processes outside the professional context.[27] In any case, I did stall on completion of this enterprise and, in addition to a good many discussions of aspects of the problems in various papers, settled for a longish chapter, "The Case of Modern Medical Practice," in *The Social System.*

Starting, perhaps, in the later 1940's there was a shift back from concern with psychological and microsociological problems to the more macrosociological, including the economic, but also perhaps a renewed sense of participation in the European scene, which was inaugurated by teaching at the Salzburg Seminar in the summer of 1948. In a sense this process culminated in a revisit to the problems of the relation between economic and sociological theory which began during my year as a visiting professor at Cambridge, England (1953-1954).

In 1946, however, I entered into formal psychoanalytic training as a "Class C" candidate at the Boston Psychoanalytic Institute. The more general intellectual grounds for my interest are perhaps clear from the above discussion, though I also had some personal reasons for seeking psychotherapeutic help. I count myself exceedingly fortunate in having had as my training analyst Dr. Grete Bibring, who had been a member of the original Freud circle in Vienna until forced into exile by the Nazi takeover of Austria. Of course, without a medical degree I could not have aspired to practice psychoanalysis, and according to the rules in force at that time I was not permitted to take control cases; indeed, I was admitted to the clinical seminars only as a special concession. However, I never had any intention of practicing.

In addition to deepening my understanding of psychoanalytic theory and the phenomena with which it dealt, this experience had the effect of contributing to my "weaning" from an overconcern with the psychoanalytic level of dealing with human problems and was hence a kind of corrective to the effect of the original reading of Freud and the early phases of my study of medical practice. Both an efflorescence of concern with more abstract and analytical problems of theory and empirical concern with less psychological areas—for example, economic and political again, and later educational—began to grow in relative strength.

I was not one of the many Harvard faculty members who was called to war service away from Cambridge. I did, however, do some teaching in the School for Overseas Administration, of which my friend Carl Friedrich was director, which administered Area and Language programs and programs for Civil Affairs officers. I taught in the fields of

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European and East Asian societies. During the latter part of the war period I served as a consultant to the Enemy Branch of the Foreign Economic Administration, dealing with the postwar treatment of Germany. I wrote several memoranda in opposition to the so-called Morgenthau Plan.

In 1944, in response in part to a very good offer from outside Harvard, I was appointed chairman of the Department of Sociology, with something of an understanding that important reorganization would take place soon. Allport, Kluckhohn, Murray and I had had important discussions on the possibility of reorganization. In 1945 there were two openings for permanent appointments in sociology. One of them went to George C. Homans, who had been in the department before leaving to serve in the Navy. The other went to Samuel A. Stouffer, who was just winding up his eminent service as director of research in the Information and Education Branch of the War Department. These events, late in 1945, made possible the faculty's action in establishing the Department of Social Relations, which opened in the fall of 1946. Stouffer became director of the Laboratory of Social Relations, an associated research organization, and I became chairman of the department, which included, besides sociology, social anthropology and social and clinical psychology. I continued as chairman for ten years until 1956. The role of Paul H. Buck, dean of the Faculty of Arts and Sciences and provost, was of outstanding importance in the establishment and development of the Department of Social Relations.

During this period I also became more active in professional affairs outside Harvard. I had served in 1942 as president of the Eastern Sociological Society, but as it was a war year the post did not entail much activity. I was elected president of the American Sociological Association for 1949 and this, of course, proved to be a much bigger job. The association was then undergoing a major organizational crisis occasioned mainly by growth in membership and activities. During my term a new office was set up, the first paid executive officer was appointed, and the association's constitution was substantially revised. After an interval of a few years I again became active in the affairs of the association, serving as chairman of the Committee on the Profession, then as secretary for five years, and finally for two years as the first editor of *The American Sociologist,* a house organ of the association dealing with matters of concern to the profession rather than "contributions to knowledge." In the 1950's I was also active in the American Association of University Professors, serving on the special committee on loyalty-security cases and for one term each on the Council and the Committee on Academic Freedom and Tenure.

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Theoretical Development, 1937-1951

For my role as teacher, especially at the graduate level, the early years of the Department of Social Relations came to be a true golden age. Opening just a year after the end of the war, the department attracted, aided by the G. I. bill, an unusually able sample of the backlog of young men whose training had been interrupted by the war. Among those who had already been at Harvard were Bernard Barber, Albert Cohen, Marion Levy, Henry Riecken, and Francis Sutton, with Robert Bales staying through the war. New arrivals included David Schneider, Harold Garfinkel, David Aberle, and Gardner Lindsey. A little later came James Olds, Morris Zelditch, Joseph Berger, Renée Fox, Clifford Geertz, François Bourricaud (from France on a Rockefeller Fellowship), Robert Bellah, Neil Smelser, Jackson Toby, Kaspar Naegele, Theodore Mills, Joseph Elder, Ezra Vogel, William Mitchell (in the Department of Government), Odd Ramsoy (from Norway), and Bengt Rundblatt (from Sweden).

In the late 1950's and early 1960's came a third wave of especially important graduate students, including Winston White, Leon Mayhew, Jan Loubser, Edward Laumann, Charles Ackerman, Enno Schwanenberg, Victor Lidz, Andrew Effrat, Rainer Baum, Mark Gould, John Akula, and, in a special collaborative relationship after completion of his formal studies, Gerald Platt. Close association with advanced students of such caliber has been one of the most rewarding features of my academic career. Such young minds cannot fail, it seems to me, to have a most stimulating effect on their teachers. My own experience has strongly reinforced my belief in the importance of *combining* the teaching and the research functions in the same organizations and roles.

A few of these direct collaborative relationships—with such people as Robert Bales, James Olds, Neil Smelser, Winston White, Victor Lidz, and Gerald Platt—resulted in collaborative publications. Other important relationships have been with David Schneider, Clifford Geertz, Leon Mayhew, and (though not as a formal student) my daughter, the late Anne Parsons.

In spite of the shifts noted above, there seems to be a certain unity in the period of intellectual interests and theoretical development running from completion of *The Structure of Social Action* to the two major books published in 1951, *Toward a General Theory of Action,* a collaborative work coedited with Edward Shils,[28] and my own *The Social System.* The most important thread of continuity lies, I think, in what came to be called the "pattern-variable" scheme.

This scheme originated as an attempt to formulate a theoretical approach to the interpretation of the professions. Clearly nothing in the

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capitalism-socialism dichotomy would do, so I turned to a famous distinction crystallized for German sociology by Toennies and used by Weber: that between *Gemeinschaft* and *Gesellschaft* as types of social organization.[29] The problem of self-interest was the initial point of reference, in a sense which posed an alternative far short of the total public interest in the socialistic sense. The professional orientation was, as I initially put it, "disinterested" (later "collectivity-oriented"), in the sense in which the physician professes to be above all concerned with the welfare of the patient. This criterion put the professions in the *Gemeinschaft* category.

The scientific component of medicine, the universalistic character of the knowledge applied to problems of illness, fitted with an extensive complex of features of modern society which Toennies and his numerous followers would have to classify as *Gesellschaft*. The obvious inference was that the Toennies dichotomy should not be treated only as variation in terms of a single variable, but also as a resultant of a plurality of independent variables. If these were indeed independent, there should be not just two fundamental types of social relationship, but a substantially larger family of such types. My suggestion was that the professional type belonged in this family but neither as *Gemeinschaft* nor as *Gesellschaft*. Important as the problem of self-interest was, it seems to me now that even more so was that of how to bring the universalism especially characteristic of cognitive rationality, and the problem of the status of nonrational emotion or affect, into the *same* analytical scheme. Quite early a dichotomous variable which I called "affectivity—affective neutrality" was formulated and incorporated in the same system which also included "Universalism—particularism."

The pattern-variable scheme underwent over the years rather complex vicissitudes which need not be detailed here. The project that produced the volume, *Toward a General Theory of Action*, however, produced a first genuine synthesis. It was initiated as a kind of theoretical stocktaking of what underlay the social relations experiment, and for this project Edward Shils and the psychologist E. C. Tolman were brought to Harvard on a visiting basis. Shils and I developed a particularly close collaboration from which came our joint monograph, *Values, Motives and Systems of Action*, which is, in a sense, the theoretical core of *Toward a General Theory of Action*. In it we developed the pattern-variable scheme as a theoretical framework not only, as I had initially assumed, for the theoretical analysis of social systems, but also for analysis of action in general, and, especially within our purview at the time, of personalities and of cultural systems. As such it was no longer a catalogue of dichotomous distinctions, but became very definitely a "system," which, however, contained seeds of much further complication than we were aware of at the time.

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This generalization and systematization seemed to constitute a real theoretical breakthrough which emboldened me to attempt a general statement on my own responsibility of the nature of a social system, including most definitely the macrosocial levels. The outcome, my book *The Social System*, apart from its codification of received sociological wisdom, rested above all on two features which might be considered original. The first of these was the clarification of the relations between social systems, on the one hand, and psychological—or personality—and cultural systems, on the other. The second was the consciously systematic use of the pattern-variable scheme as the main theoretical framework for the analysis of the social system.

Economics and Sociology Reconsidered

The two 1951 books were in some ways a culmination, but even more important they were a foundation for a major new phase. Shils's and my contribution was organized about the pattern-variable scheme, which our collaborative work generalized from the social system level to that of action generally. Robert Bales[30] and I meanwhile had had many discussions of the relations of this scheme to his scheme for the analysis of small-group interaction. These discussions became so important that we invited Shils to work with us for the summer of 1952, and the three of us produced the *Working Papers in the Theory of Action* (1953).[31]

The crucial outcome, in light of subsequent developments, was the emergence of what we now call the "four-function paradigm." Its genesis lay in a convergence between the system comprising the four elementary pattern variables and a classification that Bales had set forth in his *Interaction Process Analysis*.[32] We concluded that systems of action generally could be exhaustively analyzed in terms of processes and structures referable to the solution—simultaneously or in sequence—of the four functional problems that we called "adaptation," "system (not unit) goal-attainment," "integration," and "pattern-maintenance and latent tension-management." Though there were many defects in our formulations at that time, this basic classification has remained with me for the more than fifteen years since it first emerged and has constituted a primary reference point of all my theoretical work.[33]

A consequence, closely related to Bales's work on small groups, was an extension of the analysis to the socialization process, harking back to my study of medical practice. This extension eventuated in the book, *Family, Socialization, and Interaction Process*,[34] written in collaboration with Bales, James Olds, and others. Its main theme was that the nuclear family, emergent in modern industrial societies, could be treated as a small group and differentiated according to the four-func-

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tion paradigm, on the axes of generation and sex, in ways closely analogous to the pattern of differentiation of many of the small experimental groups with which Bales and his associates had been working. This was also, perhaps, the point at which I began to be intensely interested in the phenomenon of differentiation in living systems more generally. This interest was linked to my previous biological concerns and stressed the importance of "binary fission."[35]

This line of theorizing, a continuation of the concern with problems of nonrationality discussed above, was soon overshadowed by another which led me back to the old problem of the relations of economic and sociological theory. For the academic year 1953-1954 I was invited to serve as visiting professor of social theory at the University of Cambridge. While there I was invited to give the Marshall Lectures, sponsored by the Cambridge Department of Economics in memory of Alfred Marshall. The subject assigned to me was, specifically, the relation between economic and sociological theory.

For some years I had not been intensively concerned with problems of the status of economic theory. And, on accepting the assignment, I was not sure I could go much beyond the level attained in *The Structure of Social Action.* But the theoretical development in the interim, notably the four-function paradigm, turned out to have laid a basis for a quite new phase of analysis.

In preparation I studied thoroughly, for the first time, Keynes's *General Theory of Employment, Interest, and Money* and carefully reread large parts of Marshall's *Principles of Economics.*[36] It suddenly struck me that Marshall's extension of the basic classification of the factors of production and the shares of income from land, labor, and capital could, with the addition of Marshall's fourth factor, which he called "organization," to the three classical categories, be regarded as a classification respectively of inputs and outputs of the economy as a social system, analyzed in terms of the four-function paradigm.

This insight proved to be the starting point of a fundamental reconsideration of the problem, which was only partially carried out by the time of the delivery of the three lectures in November 1953. By unusual good luck, however, Neil Smelser, whom I had known as an undergraduate at Harvard, was then in the second year of his Rhodes scholarship at Oxford, studying economics. I sent him the manuscript of my lectures, and he responded with such detailed and pertinent comments that we arranged a series of discussions during the academic year in England. Then, the following fall, we were both back at Harvard and completed the collaboration which produced the book *Economy and Society.*[37]

We did, I think, succeed in working out a new and more generalized

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analysis of the relations of economic and sociological theory This analysis extended to the relation of the economy, conceived as a subsystem of a society, to the society as a whole. This theoretical restructuring, in addition, proved to be generalizable to a reconsideration of the other primary functional subsystems of societies. Hence it has opened up a whole new view of the structure and functioning of total social systems, of which a society is one particularly important type.

The key connection was that what economic theorists called the economy should be regarded as one of four primary functional subsystems of the society, with a primarily *adaptive* function—that is, as agent of the generation of generalized resources. Three of the factors of production and shares of income then should be regarded as, respectively, inputs from and outputs to each of the other three primary subsystems. The fourth pair—land and rent—should be treated as a special case, as it long had been in the theoretical tradition of economics. The key is the famous doctrine that the supply of land, unlike that of the other factors, is not a function of its price. This property fitted the logical requirements of the pattern-maintenance function, which we had been treating as the stabilized reference base of an action system. In the process we considerably revised the traditional economic conception of land to include not only natural resources, but also any economically significant resources that were unconditionally committed to the function of production in the economic sense, which included commitments at the value level to production. In our aspect, then, economic rationality became a category of value, not of psychological motivation.

Given that our assignments of sources of input and destinations of output among the other three subsystems were correct, and that we were able to work out comparably correct classifications and categorizations for the inputs and outputs of the other three primary subsystems, it has eventually proved possible to work out a complete "interchange paradigm" for the social system as a whole.[38] This has taken several years and has required a long series of conferences between Smelser and myself as well as with others.

This line of thought both introduced a new complication and opened up a new set of opportunities. The primary reference model of interchange for us became the Keynesian focus on the interchange between households and firms: the former are placed in the pattern-maintenance system, which made good sociological sense, the latter in the economy. Two, not four, categories, however, were involved: what economists have called "real" inputs and outputs and the monetary categories of wages and consumers' spending. This naturally raised questions on the status of money as a medium of exchange and on its other functions—for example, as measure and store of economic value.

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Monetary theory has, of course, become increasingly central in the discipline of economics, but economists and others have tended to treat money as a unique phenomenon. If the idea of a generalized interchange paradigm for the social system as a whole made sense, however, it would seem to follow that money should be one member of a family of comparable generalized media; indeed there should be four of them for the social system.

It was not too difficult to work out some of the necessary aspects of the sociology of money for it to be treated in this way; but the other media presented greater difficulties. The first success came with the attempt to treat power in the political sense as such a medium, different from but comparable to money.[39] This entailed a far more substantial reordering of conceptualizations used by political theorists than those used by economists working in the monetary context. It necessitated introducing the conception of the "polity" as defined in abstract analytical terms parallel to that of the economy—hence not as government—and concerned with collective goal-attainment to the exclusion of integration as its primary societal function. Above all, the conception of power as a symbolic medium—which is parallel to the property of money as having value in exchange but not in use—was almost totally missing in political thought, where "intrinsic effectiveness" in the Hobbesian tradition has clearly been paramount as a reference point. Nevertheless, I think a coherent paradigm of power as such a medium has been worked out (see note 38). Once this step was taken, it was much easier to extend the analysis to include two other media: "influence" and "value-commitments," these terms being used in a specifically technical sense.[40]

Media of Interchange and Social Process

By this path the stimulus of the Marshall Lectures eventually opened up an approach to not only the structural, but also the processual analysis of social systems that promises to bring treatment of their noneconomic aspects to a level of theoretical sophistication more nearly comparable to that attained for the economy and that includes the dynamics of interrelation between these other subsystems and the economy. For example, the conceptions of inflation and deflation, as these have been used by economists, now seem to be generalizable to the other three societal media and their interrelations not only with money, but also with one another. One example of the many difficulties in working this out may be cited. The monetary dynamics referred to is clearly incompatible with the idea that money is a "zero-sum" phenomenon. Credit expansion and contraction are, of course, central features of monetary infla-

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tion and deflation. Political theorists, however, have been predominantly of the opinion that power should be treated in zero-sum terms. Hence, to make money and power comparable in this vital respect, it has been necessary to investigate the basis of this contention and to show that it was untenable for my purposes.

The conception of action systems and their relation to subsystems, which had crystallized in the four-function and the interchange paradigms, strongly suggested the desirability and importance of extending the analysis. In one direction this has been pursued in considerable detail—namely, to the "general action" level, as we have called it. A first stage was central to the two 1951 books and may be regarded as a development from the two facets of the problem of rationality that emerged in my study of medical practice. This stage treated the social system as being flanked, as it were, by the psychological or personality system on the one hand, and the cultural system on the other, and interdependent with and interpenetrating with both. The logic of the four-function paradigm gradually made it clear how the "behavioral organism"—not the total concrete organism—could and should be fitted in. This extension was facilitated by the revival and expansion of biological interests, particularly through contact with Alfred Emerson and by close association with James Olds, who had gone from social psychology into the field of brain research. The functional locations of the four subsystems of action are clear and stable. They are, namely, in the adaptive position, behavioral organism; in that of goal-attainment, personality; in the integrative position, the social system; and in that of pattern-maintenance, the cultural system.

This perspective has been prominent now for some years, and attempts at a first-order breakdown of the systems other than the social have been made (for example, the paper on psychological theory for the Koch symposium and, on the cultural system, the Introduction to Part 4 of *Theories of Society*).[41] Only recently, and so far only very tentatively, has it proved possible to work out a general interchange paradigm for the general-action level.[42] An interesting convergent outcome has emerged. The categories of generalized media that have tentatively been settled on can be identified with those introduced as the "four wishes and the definition of the situation" by the social psychologist W. I. Thomas more than a generation ago. They are, respectively: the adaptive medium, parallel to money at the social system level; intelligence, which can include, in its positive form, Thomas' "wish for new experience" and, in its negative aspect, his "wish for security"; the goal-attainment medium, performance capacity, rewarded by Thomas' "recognition"; the integrative medium, affect—roughly in the psychoanalytic sense—rewarded by Thomas' "response"; and, finally, as the medium

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of pattern-maintenance process, the "definition of the situation," which, as in the other pattern-maintenance phenomena, should be and is treated by Thomas as a special case.[43]

"Structural Functional Theory?"

The concept of system, in the action field as in others, has been central to my thinking from a very early stage. With it is associated an extensive complex of empirical-theoretical problems that have entered prominently into the critical discussions of this type of theory. These concern such conceptions as equilibrium and its relations to conditions of stability and possibilities and processes of change; the status of the concept function itself; problems of "consensus vs. conflict" as characteristics of social systems; and the relation between what may be called "maintenance processes" in systems and processes of structural change, extending all the way to the conception of evolution or its opposite.

Perhaps, if I may repeat a little, my first introduction to the equilibrium problem was in the form of the Henderson-Pareto version, reinforced as it applied to the economy by Schumpeter. This form used the concept system in the sense of mechanics, with the physico-chemical systems as the model. It emphasized the conditions of stability, though Henderson was careful to point out that Pareto's conception of equilibrium was by no means necessarily static. Quite early, however, I came to be influenced by the more physiological conception of equilibrium, especially as formulated by Cannon around the concept of homeostasis.

This physiological conception articulated more directly with the functional perspective then prevalent in the thinking of social anthropologists, especially A. R. Radcliffe-Brown and his followers. Malinowski, though also known as a functionalist, was off on a different theoretical tack.[44] Radcliffe-Brown was much influenced by Durkheim and came into my orbit by that route. For a considerable time, Merton and I came to be known as the leaders of a structural-functional school among American sociologists.

Developments since the emergence of the four-function paradigm and the analysis of generalized media, in particular, have made the designation "structural-functional" increasingly less appropriate. First, it gradually became clear that structure and function were not correlative concepts on the same level—as, for example, universalism and particularism are in the pattern-variable formulation. It became evident that function was a more general concept defining certain exigencies of a system maintaining an independent existence within an environment, while the cognate of structure, as a general aspect of such a system, was process. Concern both with the maintenance of boundaries and

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other aspects of the functioning of an action system increasingly focused attention on problems of control. Money could thus be regarded as a mechanism through the circulation of which economic activities are controlled, in a manner analogous to that in which the circulation of hormones in the blood controls certain physiological processes. Such ideas articulated, in turn, with the strong emphasis in modern biological thinking that living systems are open systems, engaged in continual interchange with their environments.

Clarification of the problem of control, however, was immensely promoted by the emergence, at a most strategic time for me, of a new development in general science—namely, cybernetics in its close relation to information theory. It could now be plausibly argued that the basic form of control in action systems was of the cybernetic type and not primarily, as had been generally argued, the analogy of the coercive-compulsive aspects of the processes in which political power is involved. Furthermore, it could be argued that functions in systems of action were not necessarily "born free and equal," but had, along with the structures and processes implementing functional needs of the system, differential hierarchical relations on the axis of control.

Here developments of the cybernetic aspects of biological theory, especially the "new genetics," proved to be extremely suggestive for action theory. Particularly important was Emerson's idea that the "system of cultural symbolic meaning" played a role analogous (in the proper biological sense of analogy) to that played by genes in biological heredity. There was a notable theoretical fit between this conception and the role that had been assigned, in the theory of action, to the pattern-maintenance function and the structures and processes involved with it generally and to cultural systems particularly.

This perspective offered a way out of the endless circles of argument about the relative predominance of classes of factors in the determination of social processes and developments. For example: Was, in the last analysis, Marxian economic determinism more correct than cultural determinism? In general, such a question is meaningless, being of the same order as the old biological argument over heredity versus environment. The alternative, of course, is that action process involves combinations of factors that have different functions for the system in which they are combined, and that one major aspect of these functions is control in the cybernetic sense.

The cybernetic perspective also helped to open new possibilities for dealing with the vexing problems of stability and change in action systems. In this connection, it was possible to articulate new perspectives with my previous interest in the socialization of personality and similar themes. Insistence on a radical theoretical distinction between the

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processes by which a system-pattern is maintained—including for societies the socialization of new members—and those by which its major structure is itself altered seemed to be vindicated, as broadly analogous to the basic biological distinction between physiological processes by which the state of the individual organism is maintained or changed, and evolutionary processes involving change in the genetic constitution of the species.

Social Change and Evolution

The latter problem context has emerged into a strongly revived interest in the theory of social and cultural evolution and its continuities with organic evolution. This interest was particularly stimulated in a 1963 seminar in problems of social evolution given jointly with S. N. Eisenstadt and Robert Bellah. It has been followed up in various writings.[45] This line of interest, of course, constitutes a continuation of my concern with Weber's comparative and historical perspective, particularly in its relation to the interpretation of the nature and problems of modern society. It is also linked to more detailed problems in a series of studies of higher education on which I have recently been engaged.

A considerable part in the theoretical analysis of processes of structural change in social systems has recently been played by a paradigm that is another derivative of the general four-function paradigm. This has been labeled a paradigm of a stage in progressive structural change in a system of action, especially a social system.[46] The point of departure for this is the conception of differentiation, a process which seems on good grounds to come to focus on the binary case—that is, the division of a previous structural unit into two functionally and hence qualitatively different units. The paradigmatic case for the social system is the differentiation of the peasant type of household into a residential household and a productive agency from which income for support of the former can be derived.

For very long—for example, in the work of Herbert Spencer—differentiation has been understood to be complemented, on functional grounds, by new integrative structures or mechanisms. Partly for this reason, the newly differentiated system is also involved in new adaptive problems, very much in line with the general biological concept of adaptation as crystallized in the Darwinian tradition, but with a new emphasis on active as distinguished from passive adaptation. Finally, however, there are components of such a system that are relatively insulated from these overt processes of structural change. These components belong organically in the genetic category; action-wise, in that of pattern-maintenance. Hence a four-fold paradigm becomes appropri-

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ate at this level also. We have spoken of differentiation as focusing on the goal-attainment function, integration in the obvious place, but with a special emphasis on what we call "inclusion," upgrading of adaptive capacity as the focal adaptive category and "value-generalization" as the mode of change required to complete such a phase for the system, if it is to have the prospect of future viability.

This paradigm has played a prominent part in the spelling out of the interest in societal evolution mentioned above. The work on social evolution has been documented in a number of articles and in two small volumes written for the Prentice-Hall Foundations of Sociology series (Alex Inkeles, editor): *Societies: Evolutionary and Comparative Perspectives* (1966) and *The System of Modern Societies* (forthcoming).[47] The basis of this interest goes back to my dissertation on the nature of capitalism as a social system, which could now be more broadly redefined as an interest in the nature and main trends of modern society. This time, however, it has been approached in a broad evolutionary perspective, very much in the spirit of Max Weber, but with certain important differences from his views.

Comparative perspective has, of course, been very much in my mind, but at the same time I have been frankly concerned with the conditions and processes of modern Western development. With this in mind, and against the background of a survey of primitive and intermediate societies, I have been especially concerned with the way in which Christianity (in the context of Judaism and the culture and society of classical antiquity) set the stage for modern development. In this connection, a double set of considerations seemed to be particularly important. One was the idea that in two special cases small-scale societies—namely, ancient Israel and Greece—were able to make special cultural contributions because they became differentiated from their environments *as total societies,* on a basis which, however, did not permit them to survive for long as independent entities. Their cultures could, though, be differentiated from their societal bases and exert a profound influence on subsequent civilizations. I have called these "seed-bed" societies. In a broad sense the contributions of Israel and Greece to the modern world—especially, though not exclusively, via Christianity—are well known, but perhaps the sociology of the process is not so familiar.

The second ruling conception was that of the Christian church as a partially separated subsystem of the whole society of late Mediterranean antiquity, politically unified under Rome, which, with its strategic cybernetic position could eventually have a decisive influence on the whole process of modern development. It can be seen that the seed-bed society and the differentiated religious collectivity could be understood to serve similar functions for the long future—functions that in

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certain senses are parallel to the functions of investment for the process of economic development. I have tried to put forward this view for Christianity in two articles on its general development and significance.[48] Of course this line of analysis is, in certain respects, an extension and revision of Weber's famous interpretation of the significance of the ethic of ascetic Protestantism.

In this connection I have conceived, as so many others have, of Israel and Greece as contributing principal foundation components of what may be called the "constitutive" culture of modern civilization. These components were synthesized and changed in essential respects in Christianity. Beyond this rather commonplace position I have been concerned with elucidating the social processes by which the connection has taken place over time and with linking these interpretations with emerging interpretations of the essential elements of the modern system.

The Nature of Modern Societies

Weber's view of capitalism, like Marx's clearly rested on capitalism's relation to the industrial revolution. This view was consonant, on the whole, with the assumption that this basic change in economic organization—in close relation to technology, of course—was the most essential feature of the new society. Marx and Weber were in agreement on this main point, though they differed profoundly in their accounts of the factors involved in its genesis and in their analysis of the internal dynamics of the industrial structure.

In their different ways, both were also passionately interested in the political developments that first came to a head in the French Revolution and had immense and complex repercussions thereafter. But Marx, in his focus on class struggle, and Weber, in his focus on the process of bureaucratization, tended to relegate the democratic revolution to a place secondary to the industrial. Increasingly, it has seemed to me that the two should be placed in positions of coordinate importance. From the broad point of view of my paradigm, no matter how highly interdependent they were, one should be interpreted as being of primarily economic and the other of political significance in the analytical senses. In this sense, they can be seen as independent in focus but resting on a common base.[49]

From my interest in the professions, I came gradually to the conclusion that what may be called the educational revolution is of an order of significance for modern society at least commensurate with that of the other two. The educational revolution, of course, began substantially later, toward the middle of the nineteenth century. But with the proliferation of mass higher education in the last generation, this

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revolution has reached a kind of culmination. As a result, the occupational structure has altered profoundly—having its primary focus in the professions themselves, as specially articulated in and with the system of higher education, rather than in "line" bureaucracy.

The conception of the three revolutions—industrial, democratic, and educational—fits with the paradigm of progressive change because all three involved major processes of differentiation relative to the previous state of modern society. Moreover, all were major agencies of upgrading through the immense increase of generalized and mobile resources. All three have also clearly posed major problems of integration for the societies in which they have appeared and have necessitated major shifts of what we call value-generalization.

In accord with the logic of the four-function paradigm, it has seemed to be meaningful to seek a more fully constitutive base in some sense back of these three major transformative processes, a sense which might well include the temporal dimension. In this context, I have increasingly come to consider the possibility that the principal beginning of the modern phase of societal development lay *before* the emergence of the three revolutions—in a cultural and societal milieu that could be conceived to lay foundations common to all three. Once this possibility—as distinguished from the more common tendency to date modernity from either the industrial or the democratic revolution (or both)—was open, it seemed entirely clear that the primary locus lay in what I have called the northwest corner of the seventeenth-century European system: England, France, and Holland. In an important sense, of course, England and Holland were more closely related because both were predominantly Protestant powers with a strong economic emphasis, at that time mainly commercial. It should not be forgotten, however, that France narrowly escaped a Protestant victory and that Calvinism left a lasting influence there.

Not only was ascetic Protestantism especially strong in this whole area, but so was the aftermath of the Renaissance. The two coalesced, in a sense, in the great English and Dutch development of science in that century. The same century saw in England the first culmination in the development of common law and the establishment of the first important parliamentary regime. The France of Louis XIV, on the other hand, developed the most powerful centralized state yet seen, which served as the primary foil to the democratic revolution. The latter questioned, not the conception of the state as such—Rousseau emphasized this concept maximally—but the structure of authority by which it was controlled. Thus in terms of cultural base (especially religion and science), of legal order, and of political organization, these three countries established in the seventeenth century several of the major components

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of modernity. Nor should it be forgotten that these countries were at that time in the forefront of the institutionalization of the concept of nationality, which served as a basis for some of the conflict among them. The Dutch emancipation from Spanish rule was, of course, a major reference point.

There is an important sense in which all three revolutions presupposed this common base. The first was a kind of extension of economic differentiation and upgrading from the commercial level to the industrial—that is, the mobilization of the deeper-lying factors of production for economic upgrading. The second, intimately involved with nationalism, was a mobilization of deeper-lying factors of political effectiveness, notably the active support of the citizenry, who were no longer subjects of the monarch. The third constituted a mobilization of cultural resources in the societal interest, through the immensely complicated process of internalizing commitment to the major cultural patterns and the implementation of the attendant commitments.

In this connection, a major problem of interpretation of the trend of the modern system arose. The problem may perhaps be stated in terms of the contrast with a view which, with all their differences, may be said to be common to Marx and Weber. In a certain sense, they were agreed that the core problems of the modern system lay in power relationships. Marx located the core of this relationship in the dichotomous structure of the industrial firm, with its owner-manager versus worker structure, and then generalized this to the all-society basis of class. Weber, more realistically in the light of developments, located it in the conception of a much more differentiated firm as a bureaucratic system *not* bifurcated on a power basis, but in a diffuse and general sense controlling the actions of its participants.

The principal point of reference for a different view in my case has been the work of Durkheim, notably his conception of organic solidarity. The simplest ways to formulate the difference seem to be, first, in terms of the contrast between a predominantly associational and a hierarchical bureaucratic pattern of social structure and, second, between a more monolithic and a more pluralistic type of structure. The relation of associational structure to the problem of concentration versus dispersion of power is relatively obvious and has, of course, been very much involved in the democratic revolution. In discussing this context, the tendency has been to concentrate on the organization of government as such, and above all on central government. But in many modern societies, perhaps especially the United States, there has been a vast proliferation of voluntary associations of many different sorts. For my own purposes, a particularly important case of associationalism has been the professions, precisely because of their increasingly strategic signifi-

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cance to the occupational structure. They provide a central focus for the differences of opinion between capitalists and socialists and among the theorists who have concerned themselves with the impact of the industrial revolution. The professions clearly do not tend toward a bureaucratic type of organization, but, so far as they are involved in collective decision-making, claim considerable autonomy relative to agencies not belonging to the profession in question and act mainly as associational groups. Since the professional role is typically that of a full-time job, I have—following, indeed, Weber's and other usage—been calling this pattern "collegial." Not the least important case is the academic profession, which has over several centuries preserved a predominantly collegial pattern of organization, even though it has had to articulate with more bureaucratic ones especially in the realm of academic administration.

The problem of pluralism is somewhat more subtle. Over a wide range, differentiation of a social structure does not lead to the allocation of the personnel of the previous structure exclusively to one or the other of the resulting ones. Thus when the older peasant type of household became differentiated, the adult males continued to be members of residential households, but *also* became members of employing organizations—that is, factories and offices. The same principle applies frequently to collectivities that are units in more extensive social systems; thus various disciplinary associations are, in their corporate capacity, members of such a body as the American Council of Learned Societies. Indeed, departments as well as individuals are members of a university faculty.

Since I have attributed such importance to the process of differentiation in societal development generally, but especially in its modern phase, the phenomenon of pluralization with its distinctive features, conditions, and consequences becomes of substantive importance. In his concept of organic solidarity, Durkheim made a centrally important beginning in the conceptual analysis of this range of phenomena, a beginning on which I have increasingly been attempting to build.[50] It has become more and more evident that the phenomena in this range are of critical significance to modern society, not only in the economic sphere, but also in the articulation of occupation with kinship, ethnicity, religious structures, and various aspects of the category of community. At the same time, for a combination of reasons of ideology and intellectual history, the focus of attention and the development of conceptual tools appropriate to this area have seriously lagged behind. The tendency to focus on the two great figures of Marx and Weber and to employ either a class analysis or a bureaucratic one is indicative of this intellectual situation.

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In terms of my own intellectual experience, one particularly important point of reference could be exploited in this direction. Durkheim's approach to the analysis of the modern economy in *Division of Labor* emphasized its *institutional* regulation in the relatively informal senses, but on formal levels it stressed law more than governmental administration. The central focus was on the institution of contract and secondarily on that of property. In terms of later theory, this view directly linked the economy, as the adaptive subsystem of a society, with the integrative system that I have recently begun calling the "societal community." This linkage with Durkheim and its various subsequent ramifications lead to an emphasis on differentiatedness and pluralization of structure in direct contrast to the hierarchical emphasis on power relations that is common to both Marx and Weber.

In developmental terms, it became clear that, with the exception of the organization of the French state, the fundamental structural contributions of seventeenth-century society were of the associational-pluralistic character, notably ascetic Protestantism, common law, and parliamentarism as well as science and the rapid development of a market economy. In its capitalistic form, the industrial revolution certainly moved society farther in that direction, as did the democratic revolution. From this perspective it became clear, perhaps especially under the influence of Tocqueville, that the modern society emerging in North America was beginning to play a role in the total modern system of the twentieth century somewhat parallel to that of the European northwest corner in the seventeenth. This society owed its primary distinctive features to an associational-pluralistic emphasis and not to the sharpness and rigidity of its class discriminations nor to its especially high level of bureaucratization. In addition to decentralized governmental democracy, examples would be its federalism and separation of powers, the religious constitution of separation of church and state, denominational pluralism, and the capacity to absorb, in the sense of integration by inclusion, large immigrant religious and ethnic groups, though this absorption is far from being complete.

Spelling out the ramifications of these structural trends—assuming that they indeed exist—is a complex empirical-theoretical task, but it has increasingly become my main concern in recent years. One scholar, even when he enjoys collaboration and can connect with the work done by many others, can at best work through such complex problems only in partial and fragmentary fashion.

A substantial part of the hierarchy-power preoccupation of so much generalized social thought of the last century or so I attribute to ideological factors. Thus, classically, the socialist reaction to the capitalist conception of an economy governed by the rational pursuit of unit self-

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interest substituted rigidly centralized control by government in the public interest. In the dilemma stated by these alternatives, attention was diverted from the actual extent to which the new industrial economy was *neither* purely rationally individualistic in the sense of the utilitarian economists nor collectivistic in the socialistic sense. As Durkheim made clear, it was governed, in considerable part, by other factors. Among such factors is a normative structure, legitimized in terms of values grounded in cultural bases, notably at religious levels. In the other direction lies the affective grounding of solidarity, precisely in Durkheim's sense, in the motivational attachments of individuals to roles, to the collectivities in which they participate, and to their fellow members.

Solidarity and the Societal Community

Parallel to the rigid capitalistic-socialistic dichotomy between individual self-interest and public interest, there is a more recent dichotomy between alienation of the individual and various subcollectivities from collective solidarities, on the one hand, and expectations and demands for total absorption of the individual or relevant subcollectivity in some macroscopically conceived community, on the other. Here also a third alternative, not simply an intermediate state, is almost certainly more important than the recent and current ideological assertions would have us believe. Such alternatives presumably fit in the broad associational-pluralistic range.

Adequate theoretical resources to define these alternatives, to diagnose and further analyze the existent phenomena where they appear as well as the features of many existent structures which block them, are probably in relative terms more inadequate than were the theoretical resources for analyzing pluralistic normative components in Durkheim's time. Above all, it is necessary to establish adequate theoretical links between the psychology of the individual, the functioning of social systems in many different respects, and the grounding of the normative factors in the cultural system. One problem is to avoid simple dichotomization of the *Gemeinschaft-Gesellschaft* type, which is so strikingly parallel to that of socialism-capitalism. There is a distressing tendency among today's intellectuals to posit return to a relatively primitive level of *Gemeinschaft* as the only remedy for what are so widely held to be the malaises and the moral evils of contemporary society.[51]

The approach to this complex problem area that has been most congenial to me has been a continuation of the analysis of the process of socialization of the individual, with special reference to the interrelations between the motivational dynamics involved and the structural setting of the process seen in terms of both the social and the cul-

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tural systems. Psychoanalytic psychology at the strict personality level has provided a solid theoretical base for getting a purchase on this problem area. For understandable reasons, however, it has tended to concentrate its attention on the earlier phases of the process—especially in its classical form, the Oedipal. Even here it has needed appreciable correction and modification in the light of sociological analysis of family and kinship systems.

Psychoanalytic theory, however, has with a few exceptions (for example, Erikson on adolescence)[52] notably neglected the progressive stages of the socialization process through the various stages of formal education. It has often been content with therapy and with the ambiguous dictum that character structure has been fully laid down by the end of the first—or more usually the sixth—year of life and that what comes after that can safely be relegated to pathology or its absence.

In earlier phases of my thinking there were some beginnings of successful analysis in this area (as in the early paper on "Age and Sex" as categories of social structure,[53] later in the *Family, Socialization, and Interaction Process,* and including perhaps the social paradigm of the conditions of psychotherapy advanced in *The Social System* and elsewhere). It has, however, not proved possible to achieve a level of analytical generality in this area comparable to that obtainable in the main areas of the political, economic, legal, and even religious structures of societies.

Important progress in this direction was made by returning after some years to a consideration of problems of kinship and the incest taboo, with special focus on the significance for the associational type of society of the taboo among siblings. This consideration led in turn to an interest in the significance of "symbolic" kinship patterns in Western institutional history, especially religious orders labeled as "brotherhoods and sisterhoods." Indeed, religious celibacy could readily be conceived to constitute a case of the "investment" pattern similar to the seed-bed societies and early Christianity.[54] My views in these areas have been immensely clarified by the work of my old student and friend David Schneider on American kinship.[55]

I had previously also been engaged in a study of the relations of secondary education to social mobility, in collaboration with Florence Kluckhohn and the late Samuel Stouffer. An extension of the thought of this study backward to the elementary-school level produced important clarification of some structural reference points.[56] My empirical interests had also, as noted, extended forward on this continuum to studies of the social structure and dynamics of higher education, especially in terms of its intimate involvement with the professions.

Substantial further theoretical progress has proved to be contingent

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on the development of another generalized analytical paradigm—the classification of generalized media of interchange and the spelling out of categories of interchange among the four primary functional subsystems at the level of the general system of action, involving cultural, social, psychological, and behaviorally organic systems. As outlined above, these categories converged with the scheme of W. I. Thomas.

It had long been evident that "affect"—in something like the psychoanalytic sense, which should be clearly distinguished from erotic pleasure—should be treated as a generalized medium operating at the general action level. The problem was where it should be placed; the prominence of its psychological associations made its anchorage in the personality system especially plausible. The breakthrough came in exploring the possibility and finally making the decision to place it primarily in the social system and, of course, the latter's interchanges with the other primary systems.[57] This decision treats affect as the direct parallel, at the general action level, of influence in the social system—namely, as the primarily integrative medium. Furthermore, its status as a generalized medium makes it possible to outline a series of steps in its differentiation similar to the historic steps in the evolution of monetary exchange from barter to advanced credit systems—with the marketability of the fundamental factors of production (labor, in particular) developing at an especially crucial stage (essentially that of the industrial revolution).

The solidarity of a social system may then be thought of as a state of solvency of its "affective economy," conditioned both on the flow of instrumentally significant contributions from its members and on their motivational states of gratification, which can be thought of as the positive of which alienation is the negative. In simple social systems, these factors can be conceived to be ascribed; this is true both of primitive societies and of the socializing agencies in which, in a more differentiated society, the child is placed in the earlier stages of his socialization.

How highly differentiated his personality system must become in order to achieve high levels of gratification by full participation depends, of course, on the structure of the social and cultural milieu in which he acts. What we have called pluralization of the structure of the society is thus a major aspect of the much discussed complexity of modern life. Seen in this context, it can be suggested that the development of mass higher education, such a conspicuous and to some extent disturbing phenomenon of our time, may be regarded as a response to the societal need for personalities in sufficient numbers capable of coping with this complexity both in terms of many forms of instrumental competence and of personality integration at affective levels. The new modes of

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inclusion of individuals and subgroups in social solidarities constitute the primary focus of this set of problems of the stability and other aspects of integration of modern societies.

It is tempting to draw parallels with the states of disturbance that followed the high development of both the industrial and the democratic revolutions. In the former case, two can be distinguished: labor disturbance and that of the business cycle. Smelser has shown convincingly that a new kind of labor disturbance appeared among classes of workers who had not been injured, as had for example the hand-loom weavers, by the transition. These disturbances underlay the development of trade unionism and of the socialist movement, so far as they involved labor movements.[58] Depressions, on the other hand, raised questions of the stability of the new system at over-all systemic levels. It is also notable that attempts to deal with both categories of primarily economic disturbance were couched mainly in terms of self-interest—wages, hours, and conditions of work and the expectations of profit on the part of firms. At the same time there was a tendency, most evident in the socialist aspect and in Marxian theory, to combine these economic considerations with those of political power.

The equivalents for the democratic revolution may be said to be, on the one hand, the struggles over power and authority internal to particular political units (for example, the waves of revolution in Europe of 1789 and its aftermath, 1830, 1848, and indeed 1917-1918) and, on the other hand, the systemic waves of disturbance concerning disequilibrium in the relations among national units. The equivalent of depression here is surely war or, short of it, severely strained international relations, with a tendency for these disturbances to become increasingly generalized. Here the place of the individual self-interest of workers and entrepreneurs has clearly been taken by the collective self-interest of the Powers in their respective positions of power. At the same time, the focus on power has been modified by a highly significant integrative reference, the most prominent manifestation of which has been nationalism. Just as power struggles over economic interests have often become economically irrational, struggles over national prestige have often become politically irrational. Thus a *Realpolitiker* like Bismarck could be more rational than a nationalistic political romanticist like Napoleon III.

I suggest that what I have called the educational revolution may be interpreted as the most salient manifestation of a new phase in the development of modern society where integrative problems rather than economic or, in the analytical sense, political problems are paramount. Student disturbances would then be parallel to labor and authority disturbances because students constitute the category of persons exposed

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to the most massive problem of adjustment to structurally changed conditions. The focus of their problem is, on the one hand, not realistically in power, but in the mode of their inclusion in the course of the educational process itself—a new phase of the socialization process—and in the more general societal world after completion of formal education.

From this point of view, the radicalism of the New Left is parallel to the socialism of the labor movements and to the Jacobinism of the radical democrats. The more systemic disturbance, then, is the propagation of waves of alienation and related forms of malaise, especially among the more sensitive components of modern populations, notably the intellectuals. This concerns the stability of fulfillment of expectations of improved social solidarity in the first instance, and hence the evidences of lack of such solidarity—such as poverty, racial discrimination, crime, and war—are seen to be particularly distressing. Although it may be argued that the recent manifestations center on the integrative problem, this series of disturbances resembles the earlier ones in that there is prominent invocation of the next higher level of concern or control—in this case, that of values. This is very evident in the special prominence, especially in the more radical circles, of moral concerns.[59]

Thus, over a period of more than thirty years, an empirical interest in the problem of capitalism came to a special focus on the nature and significance of the professions. A continuing interest evolved in the broadest categorization of the nature of modern society, but was no longer put in terms either of capitalism, as such, or of the capitalist-socialist dilemma. Indeed, I am sympathetic to talk of the post-industrial society, but wonder whether it should not also be called in some sense the "post-democratic" society, a suggestion to which there would probably be considerable resistance. (I would not suggest by this term that democracy no longer counted, any more than that "post-industrial" implies that industry is obsolete.) By the same token, special attention should be paid to the system of higher education—and within it the academic profession as the structural core of the system of higher education. For the medical and academic professions I have attempted a close approximation to a standard empirical study, more so than in other aspects of my work. But in both cases I have wanted to understand the professional groups in question in the context of the wider system of which they have come to constitute particularly important parts.[60]

Higher Education as Focus

It is perhaps evident that these concerns with the main trends of development of modern society would naturally bring into sharp focus problems of the nature and current status of the system of higher edu-

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cation in modern, especially American, society. In evolutionary terms, as the culmination of the educational revolution, it had an especially salient place; special study devoted to it seemed to be far from trivial. Second, this field was particularly important in view of my long-standing interest in the modern professions, since it became increasingly clear that university-level formal training was one of the hallmarks of the professions. Training for the most prestigious of the so-called "applied" professions had developed graduate-level professional schools, which in turn became increasingly closely drawn into the universities.

The main guardian and developer of the great tradition of knowledge had become the central academic profession, mainly that institutionalized in faculties of arts and sciences. This profession of "learning itself" could be felt to be the "keystone of the professional arch" and it was to this group that I turned my primary attention. At the same time, the study of higher education offered an opportunity to continue and develop further my long-standing interest in the processes of socialization, carrying this into much later phases of the process than had occupied the primary attention of most psychoanalytically oriented students.

This interest crystallized before the Berkeley outbreak, in a project to study academic professionals in the United States, first on a pilot basis with samples from the faculties of 8 institutions and then, starting in 1967, a nation-wide sample from 116 institutions all offering four-year liberal arts programs, with or without graduate schools. This study has been generously supported by the National Science Foundation, and the main direction of research has been carried out by my collaborator Dr. Gerald Platt. As of this writing it is nearing completion.

A more autobiographical note may be added here. It is perhaps understandable that a social scientist like myself, who had become so much absorbed in matters of general theoretical concern, should be under a certain amount of tension in relation to the strong American emphasis on the importance of solid *empirical* research. Response to this pressure was surely one factor in my decision to study medical practice. This study was conceived mainly in the anthropological tradition of participant observation and interview.

With the end of the war and the entrance of Stouffer on the Harvard sociological scene survey research had achieved a position of salience in the social science world. Soon after that he and I decided to collaborate and, associating Florence Kluckhohn with us, undertook a study of social mobility among high school boys, which centered about a graduate seminar that the three of us jointly conducted. This project assembled a substantial body of data, mainly from questionnaires administered in a sample of public high schools in the metropolitan Boston area.

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A crisis supervened with the premature death of Stouffer in 1960—in the same summer in which Clyde Kluckhohn died, even more prematurely. Florence Kluckhohn and I had plans to bring out a volume and indeed the body of statistical material for which Stouffer had been primarily responsible had been carefully reworked with that in mind by the late Stuart Cleveland, but the vicissitudes of assembling the other desired contributions defeated the project.

There is, hence, a certain psychological continuity in my attempting a second round at involvement with survey research. Indeed, the use of this method had been decided upon before Platt joined the enterprise, and his training in these methods constituted one of his principal qualifications. I have considerable hope that this time it will come off, though my personal contribution will have been that of senior faculty sponsor, theoretical contributor, and critic, rather than operative survey researcher, which has been the job of Platt and the staff working with him. Especially since coming to know Stouffer well I have had a high intellectual respect for empirical social research, and very much hope that still closer alliances between these techniques and the kind of theory I have been concerned with can be worked out.

Another rather obvious continuity between the two ventures in survey research lies in the fact that they both dealt with phases of the sociology of education and both in some sense in relation to the socialization process. The mobility project was especially concerned with linking socialization, in a focus on formal education, with the occupational structure which has been so crucial to modern society since the industrial revolution. In a sense our study of higher education and the academic profession has led into a new sphere where the relations of education to the cultural tradition, coming to focus in the problem of the status of the intellectual disciplines, has taken a kind of precedence over the problems of the allocation of manpower within the occupational system, important as these are.

This is perhaps an appropriate place to take note of another context of significance for the development of my thinking, namely the American Academy of Arts and Sciences. I was elected a Fellow of the Academy in 1945 when, for a variety of reasons, including the war then just ending, it was not very active. I had attended a few "stated meetings" of the Academy, but I think a more active interest was first enlisted through *Dædalus*, my first independent contribution to which was participation in a symposium on youth in 1961. I also became a member of the Research Funds Committee of the Academy and of the Committee on the Social Science Monograph prize.

I became progressively more involved in such *Dædalus* enterprises as those dealing with the New Europe, with science and culture,

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and with the color problem[61]—and the Poverty Seminar that followed them, independent of *Dædalus*.[62] I also became for a time chairman of the Academy's Committee on Research Funds and a member of the Commission on the Future of the Academy. Finally, in 1967, I was elected president of the Academy, the first social scientist to serve in this capacity.

It was particularly congenial to me that interests of various groups within the Academy, especially through *Dædalus*, turned in the direction of the study of higher education. This has been evident in a number of ways: the Danforth Project on the governance of universities, the study of the ethical problems of experimentation with human subjects, the discussion of international problems of higher education in industrial societies, the recent studies of the status of the humanities, and the volume in which the present essay is published.[63] I have participated in the Academy's Assembly on University Goals and Governance, initiated in September 1969, as a central staff member, assigned to producing a set of generalized analyses of the nature of the current system of higher education, its place in modern society, and its possibilities of change. I have hence been in the interesting position of functioning in my Academy role mostly as a generalist student of higher education, but—in the role of research supervisor of the faculty study—I am also attempting to pin down some rather specific empirical generalizations about what academic people are really like and what makes them tick.

Participation in the work of the Academy has been particularly rewarding to me in a double sense. As an explicitly interdisciplinary organization, with active participation of members ranging across the whole spectrum of the intellectual disciplines and beyond, it has seemed to me one of the few best antidotes, organizationally speaking, to the alleged and in part actually existent trend to overspecialization in our culture, particularly its academic sector. Hence, for a scholar committed to the importance of highly generalized orientations, active participation in what has been going on in the Academy in the last decade or so has seemed to present an unusual personal opportunity for the kind of interdisciplinary action which is difficult in the local university setting. This has included exposure to stimuli which I would not otherwise have had to take account of—such as the presentations of the biologists in this volume—and an opportunity to act more positively in relation to the cognitive interests and sentiments of people involved in the immense range of different disciplines and intellectual interests represented in the Academy and various of its activities.[64]

From the more objective point of view of understanding the system of higher education and related phenomena, the Academy has increasingly come to represent a potential of generalization in the cultural

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field, in relation to the social organization of research, teaching, and application, which is not totally unique, but still probably preeminent. The very fact that such an organization can, for the time being at least, flourish in an age of allegedly rampant specialization seems to be some sort of index of the deeper concerns which are, often silently, guiding our main cultural development.

Cognitive Style and Summary of Themes

More than one commentator on the first draft of this essay has raised the question of the relation between a kind of "intellectual opportunism" and a pattern of consistency and continuity in the developments which I have outlined in the preceding pages. An attempted formulation seems more appropriate near the end of this account than near the beginning.

It is quite clear that neither in the occupational sense nor in the sense of intellectual content has mine been a meticulously planned career. The furor over the dismissal of Meiklejohn at Amherst was not foreseen when I went there, nor was the shift from biomedical to social science interests planned. Within a limited range the year at London was, but the German venture, including being assigned to Heidelberg, very definitely was not. Similarly, though going farther with economic theory was planned, involvement in sociology at Harvard clearly was not, nor was the life-long career anchorage at Harvard. Just as, when I went to Heidelberg, I had never heard of Weber, when I decided to come to Harvard I had never heard of Gay or Henderson. I was early predisposed to treat Pareto as rather indifferent and was conditioned to consider Durkheim unsound. I also had no special attraction to or knowledge of Freud until well into my thirties, and I had no special interest in the professions until nearly the same period.

There is a relation between the serendipitous element in these various career decisions at both the occupational status and the intellectual commitment level and another pattern that has continued down to the present. This is the pattern of responding to intellectual stimuli: challenges to organize association meetings and attend conferences, or, most important, to write articles on a wide variety of topics. Two early examples were a request to set up, at a meeting of the American Sociological Association (1941), a session on Age and Sex as coordinates of the role-structure of societies. It is out of this that the most widely reprinted paper I ever wrote, "Age and Sex in the Social Structure of the United States," came. The second was a request from the editor of the *American Anthropologist,* Ralph Linton, to attempt a synthesis of anthropological method in the analysis of kinship with sociological perspec-

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tive on American society. The result was the paper, "The Kinship System of the Contemporary United States" (1943), which also received rather wide attention.

It is perhaps largely in response to this kind of thing that two related images of my role in American social science have emerged. One is that—largely, I presume, by contrast with "solid" empirical research contributors—I have been held to be primarily a talented and "stimulating" essayist, writing on a variety of topics but without any genuine continuity or solidity of any kind—one might suggest an "esoterically academic" kind of journalist. The second is the attribution to me of a kind of schizophrenic dual professional personality—on the one hand this kind of journalism, on the other hand a wholly unrealistic abstract kind of formalized theorizing, with the strong implication if not assertion that the two personalities had nothing substantive to do with each other.

Professor Renée Fox has especially stimulated me (in a detailed personal communication) to reconsider the problem of continuity of development, especially at theoretical levels. I hope that my conviction that there has in fact, for over forty years, been a basic continuity has come through in the course of the preceding exposition.

In attempting to understand the nature of the psychosocial process by which this continuity has developed, I have found one parallel to be particularly suggestive. In two recent academic years I collaborated with Professor Lon L. Fuller of the Harvard Law School in a seminar under the highly permissive title Law and Sociology. In the course of it I have learned a good deal about law, and not least about the common-law tradition. From the point of view of Continental European systematists of law—an especially prominent example being Hans Kelsen—the state of common law is clearly intellectually scandalous. It allegedly consists of nothing but an aggregate of particular cases and seems almost completely devoid of principles.

Fuller, more than any other person,[65] has helped me to see that, far from the "case system" being inherently antithetical to "systematization," under the proper conditions it can be a positive vehicle of systematization. The essential point is that, since in common-law terms, with a few qualifications, courts must adjudicate *any* case put before them in a procedurally acceptable form, they have the problem, not only of rendering decisions, but also of justifying them. An appellate judicial system ensures that dubious justifications are likely to be challenged on appeal and by an intellectually critical profession, for example, in law review articles. Justification in this sense involves subsuming the particular decision, not only under specific precedents, but also under more general legal principles.

There are those in my immediate relational nexus, most notably my

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colleague George Homans,[66] who hold that the only legitimate use of the term "theory" is to designate a logical deductive system, with explicitly and formally stated axiomatic premises and, combined with appropriate minor premises, a set of deductions from them which fit empirically verifiable statements of fact. From Homans' point of view, all I have produced is a conceptual scheme which is not theory at all. A semantic issue is surely involved here but I, along with many others, have never confined the use of the term "theory" to this narrow type. I do regard it as a legitimate goal for a course of development of theory, but to say that anything short of it is not theory at all is another matter.

However that may be, two things may be said about the development outlined in this essay. First, what is at present available in my more abstract writings is not a mature system of theory in Homans' sense. Second, the process by which it, such as it is, has been arrived at has most emphatically not been one of having sat down and formulated the basic axiomatic principles and then deduced their logical implications and checked these against the known facts.

It has, on the contrary, been a process much more like that of many developments within the common law. The work of which *The Structure of Social Action* was the outcome certainly established a theoretical orientation—in my sense of theoretical—which was not a congeries of random opinions within the areas of relevance. From this conceptual scheme, if you will, as a reference base the process has been one of exploring a rather wide variety of highways and byways of empirical-theoretical problems, not, however, in wholly random succession. In this process, along with my serendipitous encounters with intellectually significant persons and influences, I have indeed reacted to quite a number of externally presented stimuli of the sort that I have characterized, especially requests to write on topics suggested by others.

In a sufficient proportion of such cases, I hope I have reacted somewhat in the manner of a competent common-law appellate judge: namely, that I have considered the submitted topics and problems in relation to a theoretical scheme, which—though its premises were not defined with complete precision and henceforth assumed as fully given in a logically complete sense—has had considerable clarity, consistency, and continuity. In a sufficient proportion of cases, it seems to me that this kind of procedure has yielded empirical insight and rounding out, extension, and revision and generalization of the theoretical scheme. At certain points this has meant intensive concern with formally defined theoretical problems, but at other points primary concern with much more empirical issues. In any case this is essentially what I have meant by the phrase "building social system theory" as used in the title of this essay.[67]

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If the above considerations throw some light on the process by which some serious continuity has been maintained, I may now say a few words, in a summary fashion about the themes which, in retrospect, seem to me to have been most important in the continuous theoretical development and the patterns of their succession.

Though a number of primary themes were involved in the theoretical patterning of *The Structure of Social Action*, notably that of the nature of the historic conceptions of economic self-interest and of economic rationality, one came to be particularly salient and has continued to be so with many variants ever since. This was what I have called the "problem of order," with reference to the human condition generally and the social system in particular. The classic early modern formulation lay in Hobbes's concept of the "state of nature" and the problem why human societies, with all their troubles, had not by and large become states of the "war of all against all." (Even with the many wars of history, the combatting units have been social systems, not isolated individuals.)[68]

My assumption throughout has been congruent with that of Hobbes in the sense that even such order as human societies have enjoyed should be treated as problematical, not assumed as obviously "in the nature of things"; in this regard perhaps I have inherited some element of Christian pessimism. Hobbes's personal solution, the "social contract" to set up an absolute sovereign who would coercively enforce order, was by the 1930's obviously unsatisfactory. But the problem remained. One of my most important reasons for linking Weber and Durkheim and, by teasing out essentially latent conceptions, Pareto was the growing insight that they had in common the recognition of the intellectual seriousness of the problem and the conviction that, in one way or another, normative factors in human action, which were analytically independent both of economic interests in the usual sense and of interests in political power, were of decisive importance.[65] Durkheim's insight about the normative components in the structure and regulation of systems of contractual relations was a truly clinching contribution to my own conceptualization; Durkheim made specific reference to Hobbes in this connection. I very much stand by the view that order in this sense is genuinely problematical, and that the nature of its precariousness and the conditions on which such order as has existed and may exist is not adequately presented in any of the views of human society which are popularly current, regardless of political coloring. There is a fundamental distinction between an intellectually competent analysis and understanding of this kind of problem and a popularly appealing ideological definition of it. They are not always at sharp variance with each other, but very generally so.

The "problem of order" is quite clearly a central focus in problem-

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formulation—in the German formulation *Problematik*—of the relations among, and the balances of factors involved in, states of stability, of tendencies to disorganization and dissolution of systems, and trends of change.[70]

The connection between the theme of order and that of convergence must be evident from the above and from earlier discussions. Insight into the problem of the theoretical significance of accounting for order and its further potentials, as well as its failures, could be considered to be a theoretical achievement. My thesis that an otherwise diverse group of theorists had converged on a common "direction of solution" of this problem, which was not obvious to the academic common sense of the time, could, on the other hand, be held to constitute a "finding."

The nature of this convergence has been sketched here and is spelled out at great length in various of my writings. It concerned the component of normative control as distinguished above all from coercive enforcement in ways which linked both with homeostatic conceptions in psychology and cybernetic conceptions over a much wider range.

As noted by Clifford Geertz in discussion of this essay, the theme of convergence did not stop with the cases intensively examined in *The Structure of Social Action*, but has continued to be a major theme of my whole intellectual career. The conviction of some kind of convergence between socioeconomic and biological thinking played an early part. Perhaps above all my concern with Freud made salient both the problem of convergence between social system theory and personality theory, and, gradually, the extent to which this was actually present. Of course such convergence often had to be teased out from what at face value were incompatible positions. The analytical distinction between personality and organism has been indistinct in most psychological thinking—indeed many psychologists today would totally deny its relevance —but, especially because of association with James Olds in the early phases of his work on the brain and with Karl Pribram, it seemed to me to be a case of convergent patterns within a framework of analytical distinctness. Similar considerations have operated in the field of the relation between social and cultural systems, for which I was primed above all by Weber, but also by my many associations with cultural anthropologists. In a sense, perhaps the most extensive convergence of all has seemed to occur under the umbrella of the cybernetic conception, with its many associations and ramifications.

The "problem of rationality" has constituted another very major thematic complex. Phrasing it as "the problem" I hope makes clear that I have not been a naïve rationalist, either in the sense of holding that virtually all human action *is* essentially rational, or that the intrusion of non- or even irrational elements should be condemned. The position,

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rather, is that to attempt to analyze the role and nature of rational components in relation to those which should not be designated as such has constituted a major focus of theoretical concerns.

It seems clear that my initial focus on problems of economic and, secondarily, political rationality—for example, in the capitalism-socialism debate—was a legitimate but limited focus. To a considerable extent I have built the above account around the relation between this focus and the two others, which in a sense are located—in the spectrum of cognitive concerns with human action—on each side of this middle one. *The Structure of Social Action,* reinforced by Freud and related influences, opened up both. For example, Pareto's conception of "logical action," strictly bound by the canons of scientific validity, opened a door into the scientific basis of the professions, of the functions of higher education, and, more generally, of "cognitive rationality" as a value pattern—as well as opening another door into the elucidation of the "psychological" nonrational.

As I have tried to make clear, for a number of years I was rather more concerned with the other alternative to economic-political rationality, namely that which linked the social system with personality in a way that proved to have highly complex relations, on the one hand to the organic complex and on the other hand to the cultural. Thus in the former context the problem of the significance of the erotic complex was salient, in the latter the problem of the role of internalized values was uppermost, starting with Freud's conception of the super-ego.

The "problem of rationality" in this context has two, or possibly three, facets. One is the question of the roles, in the determination of action, of rational and nonrational forces—for example, for Freud the ego and the "reality principle" and the id in relation to the "instinctual needs" governed by the "pleasure principle." It should be clear to the reader that my views in this area have been much less antirational than those of many other students of the problems, but I hope not naïvely rationalistic either.

The second, very vital, context concerns the accessibility of non- and sometimes irrational forces to rational understanding, that is, in cognitive terms. The intellectual movements into which I came were deeply involved in this situation. This included Freud most conspicuously, but very clearly all of my major authors, with the partial exception of Marshall. Perhaps Freud's most heroic endeavor was to set out a program for the "rational understanding of the unconscious," an entity which was, by his definition, in its very nature nonrational. This is indeed a far cry from either the rational understanding of the "rational pursuit of self-interest," or, indeed, the rational understanding of the pursuit of rationally cognitive knowledge.

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The third facet, if there is one, is the link between these two. The classical aphorism is Freud's "where Id was, there shall Ego be." We might even go back to August Comte and his slogan, *savoir, c'est pouvoir.* In what senses and within what limits does rational understanding of the nonrational—which clearly includes the physical world—open the doors to control? In the most general sense of course the answer is that it does open such doors. But this remains one of the most seriously controversial areas of the rationality complex, various aspects of which have been very central to me.

The rational component of psychotherapy shares with economic and political rationality its instrumental character. But two problems beyond this arise. The more obvious one concerns the sources of legitimacy and justification of the ends or goals in the interest of which such instrumental rationality is brought to bear. The utilitarians, and still for the most part, economists, treated consumption "wants" as given, that is, as not constituting the locus of intellectual problems for their purposes. Similarly for Freud and psychiatry mental health was an aspect of general health, and its attainment or restoration almost by definition desirable. But in both contexts, and a variety of others, it is reasonable to raise the question, paraphrasing a famous book title: "Rationality for what?" (Robert Lynd, *Knowledge for What?*)

A seductively simple solution is to say that the goals of instrumentally rational action are *basically* nonrational. But as so often, this is too simple. Weber made a major contribution to further sophistication here with his concept of "value rationality" (*Wertrationalität*), which he conceived as constituting one of the primary types of action. The essential implication, which cannot be grounded here,[71] is that the "universe of values" is not devoid of rational organization and that decisions of "commitment to" values, including their more or less direct implementation, have a rational component which is independent of instrumentality.

It has turned out that the relevance of this position, which was only most explicit in Weber, has operated in two directions, not one, in a sense somewhat analogous to the "fork in the path" encountered when following out the theoretical problems implicit in the conception of economic rationality. The more obvious of these concerns religion. In a variety of ways, problems of religion have been prominent for me almost from the beginning. It was Weber's Protestant Ethic essay which set off a major development for me, and the *common* concern of Weber, Pareto, Durkheim, and later Freud with the intellectual problems posed by religion as a human phenomenon became a major reference point in the earlier phases of my career. In the circumstances this had to involve the problem of the relation between rational and nonrational components of religion.[72]

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This concern with religion—in the role not of a *dis-* but more of an *un*believer, in the terminology of a recent Vatican conference—has been a major orientation point in my intellectual career. It was already a major aspect of my early rejection of "positivism," but at the same time has been a focus of a continuing attempt to understand the balance of the roles of rational and nonrational components in human action. Clearly, however, such a focus of intellectual concern leads one beyond the more purely cognitive problems of religion into those of moral commitment, affective engagement, and practical action.

The other ramification of the concept of "value-rationality" is in some ways more surprising. This concerns the status of the value-component in defining the relation of cognitive structures not to the clearly nonrational characteristics of the phenomena "cognized," such as the Unconscious or the "grounds of meaning" at the religious level, but to cognitive structures themselves. This has come to a head in recent years on the conception of "cognitive rationality" precisely as a *value*-pattern, not simply as a maxim of expedient "want satisfaction." The generalization of this conception was foreshadowed in the interpretation which Smelser and I put forward of the economic category of land as including a *value*-commitment to economic rationality.

The relevance of this perspective to many recent concerns, both of my own as a theorist and of society, is almost patent. It is clearly very central to the complex of higher education and its relation to the intellectual disciplines. Since the role of empirical cognitive knowledge was so central to the original formulation of the rationality problem, this concern with the value aspect of empirical cognition, both in the grounds of the cognitive validity of the knowledge mobilized in the instrumental aspects of rational action and in the cognitive problems of the justification of commitment of instrumental potentialities among goals, in a sense brings consideration of the problem of rationality full circle in that the considerations involved in the grounding of value-choices, including their more or less religious bases, are seen to be of the same order as those involved in the grounding of the validity of empirical knowledge.[73]

Perhaps I may conclude with a few words about my conception of the significance to me of the most important intellectual role models: clearly, Weber, Durkheim, and Freud, none of whom, it is important to note, I ever knew personally, though they all lived into the period of my attainment of some kind of personal awareness or perhaps of "identity," to use Erikson's term. In terms of substantive influence in the shaping of problems and of the many elements of empirical and conceptual structure which has been central to my thinking, it is quite clear that all three have been crucial. Others have of course been exceedingly important, first perhaps Pareto and Marshall, in that order, but again

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Schumpeter, Henderson, Cannon, Taussig, Piaget, and many others.

One factor in this attribution of significance is, of course the intellectual location of relevance. Equally or more towering figures in more remote fields have naturally not had the same significance in my development, even though they may have been very important on the periphery. This would have been true of Cannon, of the biological background figures like Darwin, of Whitehead, of Piaget, of Norbert Wiener, and various others. The other factor is, from such a locational perspective, the stature of these figures relative to those of directly comparable relevance.[74]

The sense in which the ideas of Weber, Durkheim, and Freud have permeated my own thinking should be clear from the above account. There remains the question of the sense in which they have been role models mainly in terms of what I have called "cognitive style." Here there emerges an important distinction between Weber and the other two. Substantively, Weber has been at least as important to me as any of the three. Stylewise, however, he was very different from the other two. He was much more, in Erikson's format, a Luther type, who, with all his immense preparation, underwent a single major transformative crisis—in Weber's case involving serious mental illness—from which he emerged as a new Weber, who, with truly dazzling "virtuosity" (a term he was fond of) produced within two or three years the great methodological essays (*Wissenschaftslehre*) and the Protestant Ethic as the opening step in a major reinterpretation of the nature of modern society seen in the broadest comparative-evolutionary perspective. It seems to me significant that, in many of his subsequent writings, Weber particularly stressed the centrality of the "charismatic breakthrough" as the most important process of religious, and more generally of sociocultural innovation and change. In suggesting that this is not the only way—connecting with the idea of the role of genius—I do not in the least mean to derogate the importance of what I regard as Weber's superlative intellectual achievement.

The cognitive style of Durkheim and Freud was quite different. I do not for a moment think that either was less intellectually ambitious than was Weber. Their method, however, was to settle on and thereby become committed to attempting a radical solution of certain definable problems in their respective spheres. For Durkheim it was a special version of the "problem of order" in the sense in which this has been outlined above. For Freud, it was the problem of rational understanding of the nonrational, with special references to the role of what he came to conceive as the "unconscious."

Clearly, in both cases, the process by which these intellectual commitments were entered into was motivationally highly complex—for

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example, in Freud's case his liberation by the death of his father. But in neither case was the maturing of his commitment a highly dramatic event, though for each it resulted in a truly seminal book, namely, Durkheim's *Division of Labor* and Freud's *Interpretation of Dreams.*

It is not quite fair, but still broadly accurate, to say that from his great breakthrough on, Weber's contribution consisted far more in a truly monumental spelling out and empirical validation of the basic insights of the critical reorientation. In the other two cases, it was a process of step-by-step development of theoretical thinking from the original problem-formulation base. In this sense there is a Weberian theory which dates from his new orientation following his recovery from his psychological disturbance—that is, about 1904-1905. In a comparable sense there is no Durkheimian or Freudian theory, but there is the documentation of an impressive process of theoretical development.

I see no reason to suggest that either of these alternative cognitive styles on the part of intellectual innovators is in any general sense superior to the other; both are critically important, but each is effective in different times and situations. Speaking personally, however, Durkheim and Freud have been my paramount role models as theoretical analysts of human action. Perhaps this has some bearing on the question of the balance between continuity and opportunism in my own intellectual history.

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2. 3 vols., 2d ed., Leipzig: Duncker and Humblot, 1916.
3. "Sociological Elements in Economic Thought, I," 49 (1935), 414-453; "Sociological Elements in Economic Thought, II," 49 (1935), 645-667.
4. "Wants and Activities and Marshall," *Quarterly Journal of Economics,* 46 (1931), 101-140; "Economics and Sociology: Marshall in Relation to the Thought of His Time," *Quarterly Journal of Economics,* 46 (1932), 316, 347.
5. Pareto gave the French edition of his book the title *Traité de sociologie générale.* (The Italian was *Trattato . . .*) It has always seemed to me unfortunate that the English translation, published some years later than my work, bore the title *The Mind and Society.*
6. This paper was never published as such, but its main substance, after considerable revision, was published as the three chapters on Pareto in *The Structure of Social Action* (New York: McGraw-Hill, 1937).
7. George Simpson, trans., New York: Free Press, 1964.
8. The first part of this book that I actually committed to paper concerned Durkheim's early empirical work and dealt with the Division of Labor and his (subsequently) very famous study of suicide (Chapter VIII).

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9. Thus Sorokin, whose *Contemporary Sociological Theories* was the most widely used compendium in the field in the 1930's, treated Pareto, Durkheim, and Weber as belonging to entirely different schools, and did not once mention a relation between any pair of them. Marshall, of course, he did not discuss at all because he was labeled an economist, not a sociologist.

10. In retrospect it seems to me that this experience was, even apart from the substantive importance of Kant for my problems, especially important training for my later work. It was reinforced by a seminar and oral exam on the same book under Karl Jaspers at Heidelberg in 1926. The importance lay in the fact that I undertook the detailed and repeated study of a great book, the product of a great mind, to a point of reaching a certain level of appreciation of the nature of its contribution, and not being satisfied with the myriad of current rather superficial comments about it. This experience stood me in good stead in working with the contributions of my own authors and coming through to what I felt to be a high level of understanding of them in the face of many distorted interpretations current in the secondary literature, some of which were widely accepted.

    After the Pugwash Conference of 1967, I. I. Rabi commented to me on the importance of contact with the operation of what he explicitly called "really great minds." He spoke of his own good fortune in his student or early post-student days of being able to work in the laboratories of Pauli and Bohr. I did not have anyone of that caliber among my teachers in the flesh—though possibly Jaspers was—but I had a good many teachers who had that appreciation and I was able to develop it for myself through intensive study of the writings of great minds. The course on Kant was my first introduction to that experience.

11. Bernard Barber, ed., *L. J. Henderson on the Social System* (Chicago: University of Chicago Press, 1970).

12. New York: Macmillan, 1926.

13. Hans Vaihiger, *The Philosophy of "As if,"* trans. C. K. Ogden (New York: Barnes & Noble, 1952).

14. L. J. Henderson, *Pareto's General Sociology: A Physiologist's Interpretation* (Cambridge, Mass.: Harvard University Press, 1935).

15. Claude Bernard, *An Introduction to the Study of Experimental Medicine,* trans. H. C. Green, repub. ed. (New York: Daler, 1957).

16. W. B. Cannon, *The Wisdom of the Body* (New York: Norton, 1932). In the discussion at the Bellagio conference, when this essay was first presented, a question was raised about the seriousness of this exposure to biology. I was greatly pleased when Professor Curt Stern said: "May I make one very short point in regard to Amherst that not everybody might know. At Amherst, biology was taught at a very advanced, even a graduate level, although Amherst did not give doctors' degrees. These were highly distinguished people, and probably their influence was greater than it would have been had Professor Parsons gone to another college with good but less distinguished professors."

17. New York: Free Press, 1951.

18. It should be remembered here that social scientists had been forced to expend much ingenuity and energy on fighting off pressures toward illegitimate and premature biological "reductionism."

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19. I was married in 1927 to Helen B. Walker, whom I had met as a fellow student at the London School of Economics. She has worked at Harvard for many years, most recently in administration of the Russian Research Center.

20. Thus he was extremely hostile to President Roosevelt, whose general policies I personally supported.

21. A note on the then, as now, prevalence of the belief that the word was "publish or perish" and that the criteria of conformity were to publish as soon as possible and as much as possible. I can testify that the responsible advice which I received from senior faculty members at Harvard did not fit the formula. It was uniformly to take as much time as necessary to do the best job of which I was capable. Of course, it helped immensely that such people as Henderson, Gay, and Wilson knew what I was doing and had seen samples of it. Another senior critic who should also be mentioned in the same vein was the late A. D. Nock.

    As in the case of most formidable personalities, there was an underground reaction to Henderson. He wore a reddish beard and was, behind his back, widely known as "Pinkwhiskers." My sessions with him took place in the late afternoon and I generally went directly home and told my wife what Pinkwhiskers had said. I remember worrying that my children would pick up the name and on some occasion say directly to Henderson: "Are you the Pinkwhiskers my daddy talks about?"

    It is fortunate that, between the first and final drafts of this paper, there has appeared *L. J. Henderson on the Social System,* edited by Bernard Barber. It contains most of Henderson's sociological writings, with a long and informative introduction by Barber. Barber's introduction tells in considerable detail the story of Henderson's involvement in the social science scene at Harvard and the various people in it with him.

22. Perhaps I may be pardoned for noting that, though I felt I was exceedingly fortunate to get the book published in the first place without subsidy and to have it republished in 1949 by The Free Press, it has maintained a steady substantial sale for over thirty years; indeed, just thirty years after the original publication a paperback edition was brought out which has sold well. In this matter I owe a particular debt to Jeremiah Kaplan, who practically "was" The Free Press, for his imagination in bringing out the 1949 reprint and sticking with this and a considerable series of my other publications. Without Kaplan, and his principal adviser Edward Shils, the postwar efflorescence in sociological publication, a wave on which I rode, probably could not have occurred, at least not so soon.

23. I have barely noted above that in my undergraduate days a comparable basic decision was made, namely to go into social rather than biological science. My switch to social science—a qualified economics initially—was associated with my father, who, during my development, was a college teacher and administrator. When I was a student in college my father was president of Marietta College in Ohio. He had begun his career as a Congregational minister and was very much involved with the then important "social gospel" movement, which, it is now clear, had much to do with the origins of sociology in this country.

24. Reprinted in Barber, ed., *Henderson on the Social System.*

25. See my paper, "Social Structure and the Development of Personality: Freud's Contribution to the Integration of Psychology and Sociology," in *Social Structure and Personality* (New York: Free Press, 1964).

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26. See my paper, "The Problem of Controlled Institutional Change," reprinted in *Essays in Sociological Theory*, rev. ed. (New York: Free Press, 1954).

27. Possibly one precipitating factor in this diversion was the death, in 1938, of my father-in-law, Dr. W. D. Walker of Andover, Massachusetts, at the age of sixty. Dr. Walker was a particularly fine type of general practitioner of medicine and had been exceedingly helpful to me in the working out of the fieldwork phase of my medical study and in general discussions of the medical scene. At the same time, he was sufficiently old-fashioned not to "take much stock" in my more esoteric psychological interests.

28. Talcott Parsons and Edward Shils, eds., *Toward a General Theory of Action* (Cambridge, Mass.: Harvard University Press, 1951).

29. Ferdinand Toennies, *Community and Society* [*Gemeinschaft und Gesellschaft*], trans. and ed. Charles P. Loomis (New York: Harper & Row, 1963).

30. Bales had been one of the few graduate students in residence during the lean war years. As a junior faculty member he began his notable program of the experimental study of small human groups.

31. New York: Free Press, 1953.

32. Cambridge, Mass.: Addison-Wesley, 1950.

33. This scheme was explicated in Chapter III of *Working Papers in the Theory of Action*. Shorthand for it became the "A,G,I,L" scheme.

34. Talcott Parsons, Robert Freed Bales, James Olds, Morris Zelditch, and Philip Slater, *Family, Socialization, and Interaction Process* (New York: Free Press, 1955).

35. This interest was, as noted above, strongly stimulated by participation in the continuing conference on theories of systems, organized by Drs. Roy Grinker and John Spiegel. I was particularly influenced, in that conference, by the contributions of the Chicago biologist Alfred Emerson. See Appendix to *Family, Socialization, and Interaction Process*.

36. New York: Harcourt, Brace, & World, 1936 (Keynes); 8th ed., London: Macmillan & Co., Ltd., 1925 (Marshall).

37. New York: Free Press, 1956.

38. The interchange paradigm appears as an appendix to my article "On the Concept of Political Power," *Proceedings of the American Philosophical Society*, 107 (June 1963), reprinted in *Sociological Theory and Modern Society* (New York: Free Press, 1967).

39. *Ibid.*

40. This analysis is in two of my articles: "On the Concept of Influence," *Public Opinion Quarterly* (Spring 1963) and "On the Concept of Value-Commitments," *Sociological Inquiry*, 38 (Spring 1968), both reprinted in *Politics and Social Structure* (New York: Free Press, 1969).

41. See the Introduction to Part IV of Talcott Parsons, Edward Shils, Kasper D. Naegele, and Jesse R. Pitts, eds., *Theories of Society* (New York: Free Press,

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1961) and my essay, "An Approach to Psychological Theory in Terms of the Theory of Action," in Sigmund Koch, ed., *Psychology: A Science,* III (New York: McGraw-Hill, 1959).

42. See the Appendix to "Some Problems of General Theory in Sociology," in John C. McKinney and Edward Tiryakian, eds., *Theoretical Sociology: Perspectives and Developments* (New York: Appleton-Century-Crofts, 1970).

43. W. I. Thomas, *The Unadjusted Girl* (Boston: Little, Brown, 1923).

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45. "Evolutionary Universals in Society," *American Sociological Review,* 29 (June 4, 1964), reprinted in *Sociological Theory and Modern Society;* "Christianity," in David Sills, ed., *International Encyclopedia of the Social Sciences* (New York: Macmillan and Free Press, 1968); *Societies: Evolutionary and Comparative Perspectives* (Englewood Cliffs, N. J.: Prentice-Hall, 1966); *The System of Modern Societies* (Englewood Cliffs, N. J.: Prentice-Hall, forthcoming).

46. See *Economy and Society,* Chapter 5.

47. See note 45, above.

48. See "Christianity," in Sills, ed., *International Encyclopedia,* and "Christianity and Modern Industrial Society," in Edward Tiryakian, ed., *Sociological Theory, Values, and Sociocultural Change: Essays in Honor of Pitirim A. Sorokin* (New York: Free Press, 1963).

49. There are complexities of "crossover" involved. Thus bureaucracy in economic production is a harnessing of *analytically* political components in an economic interest, and the constituency aspect of political democracy is a comparable harnessing of integrative components in the interest of government.

50. See my essay, "Durkheim's Contribution to the Integration of Social Systems," in Kurt Wolff, ed., *Emile Durkheim, 1858-1917: A Collection of Essays with Translations and a Biography* (Columbus: Ohio State University Press, 1960), reprinted in *Sociological Theory and Modern Society.*

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52. Erik Erikson, "Youth: Fidelity and Diversity," *Dædalus* (Winter 1962), pp. 5-27.

53. "Age and Sex in the Social Structure of the United States," *American Sociological Review,* 7 (October 1942), 604-616, reprinted in *Essays in Sociological Theory.*

54. See "Kinship and the Associational Aspects of Social Structure," in Francis L. K. Hsu, ed., *Kinship and Culture* (Chicago: Aldine Press, forthcoming).

55. David M. Schneider, *American Kinship: A Cultural Approach* (Englewood Cliffs, N. J.: Prentice-Hall, 1968).

56. See my articles, "The School Class as a Social System: Some of Its Functions in American Society," *Harvard Educational Review,* 29 (1959), 297-318, reprinted in *Social Structure and Personality,* and, with Gerald M. Platt, "Higher Education, Changing Socialization, and Contemporary Student Dissent," in Matilda Riley, ed., *A Sociology of Age Stratification* (New York: Russell Sage Foundation, forthcoming).

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57. Professor Renée Fox, in a commentary on the original draft of this essay, suggested that the decision to place "affect" primarily in the social system was effectively made much earlier, namely in formulating the pattern-variable, "affectivity vs. affective neutrality." The decision, new or old, has been hotly contested by two of my ablest young collaborators, Victor Lidz and Mark Gould.

58. See Neil Smelser, *Social Change in the Industrial Revolution* (Chicago: University of Chicago Press, 1959) and *Essays in Sociological Explanation* (Englewood Cliffs, N. J.: Prentice-Hall, 1968).

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60. Preliminary findings are reported in Parsons and Platt, "The American Academic Profession."

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62. Daniel P. Moynihan, ed., *On Understanding Poverty* (New York: Basic Books, 1969); James L. Sundquest, ed., *On Fighting Poverty* (New York: Basic Books, 1969).

63. *Dædalus* issues on "The Embattled University" (Winter 1970), "Ethical Aspects of Experimentation with Human Subjects" (Spring 1969), and "Theory in Humanistic Studies" (Spring 1970).

64. A broad survey of some of these interests and activities can be gleaned from the annually published *Records* of the Academy. My first two annual reports as president are in the 1968 and 1969 volumes.

65. Lon L. Fuller, *The Anatomy of the Law* (New York: Praeger, 1968).

66. George C. Homans, *Social Behavior: Its Elementary Forms* (New York: Harcourt, Brace & World, 1961); *The Nature of Social Science* (New York: Harcourt, Brace & World, a Harbinger Book, 1967); "Contemporary Theory in Sociology," in Robert E. L. Faris, ed., *Handbook for Modern Sociology* (Chicago: Rand McNally, 1964).

67. Another important focus of this process lies in the teaching role. This is analagous to the common-law courts in that the teacher, especially at the advanced undergraduate and graduate level, is obligated, within rather wide limits of definition of his sphere of competence, to try to deal with questions raised by his students —in class, perhaps especially during seminar discussions; in term papers, honors papers, or other theses; and in personal conferences. Within limits the students, not the teacher, formulate the problems. If he deals with the students with competence and integrity, he must continually refer them to the generalized theoretical structure of the relevant bodies of knowledge. I have long thought that the enormous stimulus of these interchanges, both to good theoretical thinking and to incentive to be empirically informed and sound, constitutes one of the main reasons why too great a separation of the functions of research and teaching is unlikely to be healthy for the academic enterprise.

Certainly, in my own case, interaction with a succession of generations of in-

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tellectually able and curious students has constituted a major stimulus to the development of my theoretical thinking and acquisitions of empirical knowledge. The exceptional graduate students named earlier in this essay have played an especially important role in this respect—in a gratifying proportion of cases extending well beyond their student days.

68. It may be of interest to note that I took a Kantian approach to the problem of order. Very broadly, with respect to the epistemology of empirical knowledge, Hume asked "*is* valid knowledge of the external world possible?" and came out with, by and large, a negative answer. Kant, on the other hand, posed the question in a more complex way. He first asserted that "we in fact *have* valid knowledge of the external world" then proceeded to ask "*how* is it possible?", that is, under what assumptions? Similarly some social theorists have wondered whether social order was possible at all, and often denied its possibility. I, on the other hand, have always assumed that social order in fact *existed*, however imperfectly, and proceeded to ask under what conditions this fact of its existence could be explained.

69. That the same was true of Marshall, in the sense that he assumed it but did not let it obtrude too much into his technical economics, goes almost without saying. One may say that if there ever was a late Victorian, "evangelical" Englishman, Marshall was one.

70. I have been widely accused by critics of being a last-ditch defender of order at any price, the ultimate price usually being interpreted to be fascism. Fortunately the more preceptive of the critics have seen order as a problem, not as an imperative.

71. See my article, "The Sociology of Knowledge and the History of Ideas," in Philip Wiener, ed., *Dictionary of the History of Ideas* (New York: Scribner's, forthcoming).

72. Fairly early in my Harvard teaching career I introduced a course in the Sociology of Religion which continued for at least two decades, in the later years in collaboration with Robert Bellah.

73. If, without being accused of being a "racist," I may venture to quote an old Negro spiritual, I think the phrase "there's no hiding place down there" sums up the situation admirably. By "down there" in the present context I mean the positivistic view of the total cultural self-sufficiency of science, which has been alleged to have no "deeper" connections with any components or problems of human orientation outside itself.

74. I am reminded here of a geographical case which I have personally experienced a number of times. Seen from the valley of Chamonix, the Mont Blanc massif is clearly the most important mountain mass in that region of the Alps. If, however, one moves from Chamonix and its neighborhood, not only to Geneva, but beyond to the slopes of the Jura, the preeminence of the massif becomes overwhelmingly salient—providing weather permits visual perception. It is in an analogous sense that I think my three major figures constitute the highest "peaks" of the "range" of intellectual achievement in the field most relevant to me and in the time. This is by no means to deny the importance of the rest of the range.

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CURT STERN

The Continuity of Genetics

The Dawn of Classical Genetics: Mendel's Laws

When in 1865 Gregor Mendel presented his paper "Experiments on Plant Hybrids" he reported on various types of pea plants and gave ratios such as tall to short plants or yellow to green seeds in the offspring of hybrids. When only a single pair of contrasting traits was considered, ratios like 3:1 or 1:1 among the progeny of crosses were presented, and when two or more pairs of traits were followed simultaneously, ratios like 9:3:3:1 or 1:1:1:1 and even more complex ones were described. These ratios were expressions of empirical data and as such were open to discussion and interpretation. Mendel himself went beyond the empirical realm and provided a rational framework from which the facts could be deduced. He distinguished between the traits which he observed and the conceptual *Elemente* which were in the cells of his specimens and were responsible for the formation of the traits. He recognized the existence of pairs of these elements which corresponded to the pairs of contrasting traits, and he conceived of the existence of many pairs of elements in the cells of his plants corresponding to the many pairs of observable contrasting traits.

The ratios which Mendel had determined appeared as the consequences of two simple processes postulated by him: segregation and independent recombination of the elements. The first of these consisted in the separation of the partners of each pair of elements during formation of germ cells, or, more accurately stated, during formation of the ovules and pollen. Though the two contrasting elements of a pair are present together in the vegetative cells of a hybrid, only one of them is assigned to any one ovule or pollen produced by the hybrid. Specifically, Mendel stated, one half of the germ cells receive that element of a pair which came to the hybrid by way of the ovule from which it developed after fertilization and the other half of the germ cells receive that element which came to the hybrid by way of the pollen grain which had fertilized the ovule.

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The second process which Mendel postulated took into account the fate of more than a single pair of elements. If A^1 and A^2 represent the elements of one pair and B^1 and B^2 those of another, a hybrid may be symbolized as $A^1A^2B^1B^2$. Segregation demands that all germ cells of the hybrid receive either A^1 or A^2 and either B^1 or B^2. But which of the A^1A^2 elements will be associated with B^1, which one with B^2? Mendel found that A^1 and B^1 are as frequently together as A^1 and B^2, and A^2 and B^2 as frequently as A^2 and B^1. Thus, independent recombination occurs, based on a process which gives no preference to any one association. In 1909, more than forty years after Mendel's exposition, his elements were named "genes" by the Danish geneticist Wilhelm Johannsen, who derived the term from William Bateson's 1906 designation of the science of heredity and variation as "genetics." Mendel had postulated the existence of genes without being able to assign them a physical basis. Johannsen could have done so but insisted that they should be regarded as no more than operational units.

Four years after Mendel's report in Brünn, Friedrich Miescher in Tübingen discovered a new chemical substance in the nuclei of pus cells and later in the nuclei of fish sperm. This substance, which he called nuclein, was deoxyribonucleicacid, DNA. It is nearly certain that Mendel never knew of Miescher's finding and that Miescher never heard of Mendel. If they had, it would not have helped them much in their diverse endeavors. Even though it was speculated early that DNA was the heredity substance, for a long time no connection was established between the specific genes of Mendel and Miescher's chemical analysis of nuclei en masse. Only slowly did the cellular role and the biochemistry of DNA become clarified, and not until three quarters of a century after Miescher's initial work was a belief in the genetic significance of DNA based on more than general arguments. We shall return to this late union of genes and DNA.

It is well known that Mendel's paper did not make an impression on his contemporaries and remained virtually unknown until the turn of the century, when its findings were rediscovered. Three investigators, without knowing of Mendel, experimented with crosses of varieties of plants and subsequently found references to Mendel's paper. With the publication of their data together with their acknowledged "Mendelian" interpretation the field of genetics became permanently established. Although the gap in hybridization studies which lasted from 1865 to 1900 seems to belie the theme of this essay, "The Continuity of Genetics," there was no gap in the wider pursuit of genetic tasks. During this period the fundamental facts of fertilization and of cell and nuclear division were established and incorporated into theories of heredity and development. Soon after 1900 the stream of efforts in cell studies was

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joined by that in hybrid analysis, and in the following decades the river of genetics sent branches to all areas of biology and received tributaries from them.

It might seem a strange coincidence that three separate individuals succeeded in the same year of 1900 to rediscover the Mendelian laws. In reality the part played by each of these men was different. Of the three, Hugo De Vries, in Holland, was the first to publish an account of his findings. This prompted Carl Correns, in Germany, to write up an account of his own work within a day of receipt of a reprint of De Vries' paper. Erich von Tschermak, in Austria, composed his account after seeing the papers of his two predecessors. All three investigators independently retrieved Mendel's forgotten paper, references to it being easily found in a well-known book by the botanist Wilhelm Focke, published in 1881 (this compilation surveyed all previous work in plant hybridization but did not grasp the significance of Mendel's work).

With the rediscovery of Mendel's publication the common merit of De Vries, Correns, and Tschermak ends. De Vries and Correns not only claimed that they had discovered the essential facts and developed their interpretation before they found Mendel's article, but they demonstrated in their own publications that they fully understood the essential aspects of Mendel's theory. On the other hand, Tschermak's papers of 1900 were inadequate in the analysis of their own data and were lacking an interpretation. I remember well a conversation with T. H. Morgan in the middle twenties. Morgan judged that Correns had shown clearly by his later work that he could have founded "Mendelism" without seeing Mendel's paper, and that Tschermak had shown that he was unlikely to have been an independent discoverer. Morgan was inclined to judge De Vries' independence as not proven by the later work of this undoubtedly great scientist.

The rediscovery of the Mendelian laws in 1900 was a far lesser accomplishment than Mendel's original feat. This has been stressed by Correns. In a later account of his part in the "confirmation of that which had been discovered more than 30 years before" he wrote: "And through all that in the meantime had been discovered and thought (I think above all by Weismann), the intellectual labor of finding out the laws anew for oneself was so lightened, that it stands far behind the work of Mendel."[1]

The process of fertilization of the egg which initiates the development of all sexually reproduced organisms had remained obscure from the dawn of biology until 1875. In that year Oskar Hertwig at the age of twenty-six observed for the first time the essentials of fertilization—in the sea urchin. In this organism the small transparent eggs are spawned into sea water, where a sperm enters an egg. After entrance the sperm's head, its nucleus, swells up and assumes an appearance similar

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to the nucleus of the egg. The two nuclei then coalesce into a single unit as fertilization occurs. It was not long until the essence of Hertwig's observations was confirmed in other animals as well as in plants: fertilization consists of the complete union of two and only two sexual cells, and it involves the union of the two parental nuclei.

Mendel himself had been concerned with the nature of fertilization. In his historic paper on plant hybridization he wrote that "according to the opinion of famous physiologists" reproduction in higher plants involves the pairwise union of ovule and pollen cell to form a single cell. He found support for this view in his experiments with peas, where among the offspring of hybrids both original parental types recur in equal numbers. He specifically rejected the idea that the ovule plays only the role of a foster mother of the pollen cell, since in that case the hybrid would resemble the pollen plant exclusively or very closely. "A thorough proof," he wrote, "for complete union of the content of both cells presumably lies in the universally confirmed experience that it is immaterial to the form of the hybrid which of the parental types was the seed or pollen plant."

Notwithstanding these valid considerations and the opinion of famous physiologists, Mendel felt the need to reexamine the view that only a single pollen grain suffices for fertilization. In 1869 and 1870, although he was troubled by an eye ailment and his time for experiments was greatly curtailed by his duties as abbot of his monastery, he could not bring himself to postpone a crucial experiment. He placed single pollen grains on the stigma of flowers of *Mirabilis Jalapa,* the four-o'clock, and obtained well-developed seeds and from them plants which could not be distinguished in any way from those produced by ordinary pollination. He thus refuted the opinion of Charles Darwin and of the French plant hybridizer Charles Naudin, who had come close to being an independent discoverer of Mendel's laws. Naudin and Darwin had maintained that a single pollen grain does not suffice for fertilization of the ovule. (A detailed analysis of Naudin's and Darwin's views will appear in a forthcoming publication by Robert Olby.)

In his experiments Mendel anticipated Hertwig's observations with sea urchin eggs, yet Mendel's fundamental findings of the sufficiency of a single pollen grain remained even less accessible to other investigators than his pea experiments. The latter, after all, had been published in a scientific journal (the *Transactions of the Brünn Natural History Society*), while the former were reported solely in letters to the Swiss botanist Carl Nägeli, letters which remained unknown to everyone else until their publication early in the twentieth century. In one of these letters Mendel refers to another experiment under way, ingeniously designed to find out whether two pollen grains may simultaneously participate in fertiliza-

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tion. No report on the results of the experiment has come down to us.

The volumes of egg and sperm of a given species are nearly always very disparate. The egg is a large, nucleated cell rich in cytoplasm and laden with reserve materials, the sperm a very small cell consisting mostly of a condensed nucleus and possessing only a small amount of cytoplasm. Clearly the quantitative contributions of egg and sperm are unequal. Why then does not the offspring of two parents resemble its mother more than its father, instead of, by and large, resembling both parents to the same degree?

The answer was provided by Hertwig's microscopic observations and those of his followers and contemporaries in the 1880's. Since the two uniting nuclei involved in fertilization are more or less alike in size and general content, *they* must be the bearers of the hereditary material which the new individual obtains in equal amounts from his two parents. The extranuclear cytoplasm of egg and sperm and its content apparently is neutral as far as inherited traits are concerned.

The visible equivalence of egg and sperm nucleus was resolved further. During division the otherwise diffuse nuclei of cells were shown to form well-defined threads or loops, which were later called "chromosomes" since these bodies preferentially take up certain stains. The Belgian zoologist and parasitologist Edouard van Beneden discovered that *Ascaris*, the large intestinal round worm of the horse, had only four chromosomes in its fertilized eggs, one of the very few species with so small a number. In 1883 van Beneden showed that in fertilization the egg nucleus and the sperm nucleus alike contain two chromosomes; not only are the nuclei in bulk contributed equally by the two parents, but specifically the countable, formed elements—the chromosomes. It was reasonable to conclude that it is not the diffuse content of the nuclei but their chromosomes which are the bearers of the hereditary material.

Independently of van Beneden's publication the presumably genetic role of the chromosomes had been pointed out in a brilliant, brief pamphlet, "On the Significance of the Nuclear Division Configurations" (1883). Wilhelm Roux, the author of this "hypothetical discussion" (as the title page termed it), asked himself what the meaning was of the complicated processes of nuclear division—mitosis. The work of a galaxy of students on the fine structure of the cell—work made possible by great advances in optical theory and the engineering of optical instruments and by the introduction of fixation and staining procedures for the study of cells—had culminated in the finding that each chromosome is split lengthwise during mitosis and that the two resulting daughter chromosomes subsequently move to opposite poles and so become included in two different cells. The cytoplasm of a dividing cell is usually simply constricted into two portions, each of which becomes a new cell. Why

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does the nucleus not divide itself up in a similar rough fashion, instead of arranging its contents in long threads—the chromosomes—and then cleaving them lengthwise?

Roux's approach was frankly teleological. The founder of *Entwicklungsmechanik*, the causal analysis of developmental processes, he was willing to ask questions about the purpose of a biological phenomenon which were complementary to questions about its causal antecedents. "Let us consider the task," he wrote, "of dividing into two halves a mixture of different substances in such a way that in each half is found one half of each of the different substances." If the number of different substances is very large, and the amount of each very small, then division of each "granule" representing one of the substances and separation of the sister granules into opposite locations would be a suitable method. Moreover, the task of dividing and distributing the sister granules would be eased greatly if many granules were arranged together longitudinally, forming one or several threads, dividing lengthwise whole threads and distributing their daughter threads into two separate assemblies. "Our purpose was the halving of a mixture of substances not just in terms of its total mass but also in terms of each separate quality . . . If the purpose discussed by us is likewise that of mitosis, then all those marvelous processes of mitosis would be recognized as fully purposeful. Conversely, since we cannot assume . . . that a process can be useless that is so generally present, that requires so much time and energy and that is so complicated and difficult to acquire, we must admit a certain probability that our postulated purpose is also the purpose of mitosis."[2]

For Roux the function of the extraordinary multiplicity of nuclear qualities consisted in their importance for physiological, behavioral, and developmental processes. Explicitly the word "heredity" itself cannot be found in the pamphlet, but its essence is implied. In the final pages Roux stresses that the apparent homogeneity of the chromosomal substance of the nucleus should not deceive a critical observer. "One must be aware," he wrote (translated somewhat freely), "that we look at the molecular processes of the cell in a way similar to looking at a factory from a balloon that floats suspended in the highest regions of the atmosphere: the diameters of the parts are a millionfold smaller than the distance from which we see them and the most diversified objects therefore appear homogeneous."[3]

At the same time that Roux recognized the chromosomes as the bearers of the submicroscopic units which are transmitted from cell to cell and which control the fundamental life processes of the organism, August Weismann came to a similar conclusion and explicitly attributed to the chromosomes the role of bearers of hereditary units. His elaborate theoretical framework included a point of view which was diametrically

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opposite to the one held by most biologists from antiquity up to and beyond Charles Darwin. The old view derived the hereditary constitution of the germ cells from messengers sent out by the body parts of the organism. Darwin, for instance, in his "provisional hypothesis of pangenesis" conceived of each organ sending out particles into the blood stream which then were assembled in egg and sperm. This hypothesis implied the possibility of the inheritance of acquired characteristics. The experiences of an organ would mold the nature of its "gemmules," and the hereditary properties of the germ cells would reflect the acquired properties of the organ. Weismann's new view looked at the changeable individual as only a passing expression of the invariant content of the generations of germ cells. Instead of the egg being considered the hen's way of making more hens, the hen was being regarded as the egg's way of making more eggs. The principle of conservation of the hereditary material through the chain of life was applicable only to the contents of eggs and sperm—the germ plasm—not to individuals. There was no inheritance of acquired characteristics.

When Weismann erected his theoretical edifice he had not heard of Mendel, but when Mendel's work became known, Weismann was jubilant. "The Mendelian laws are a confirmation of the basic tenets of the theory of the germ plasm."[4] Indeed, Mendel's "elements" were observed to segregate unchanged in their properties by the nature of the individual which had carried them from fertilization to maturity. Crosses of tall by dwarf pea plants yielded tall hybrids. When these produced pollen and ovules, half of them carried the unchanged gene for tallness, which had been present in the tall hybrids in the same cells which also held the gene for dwarfness; the other half of the pollen and ovules carried the unchanged gene for dwarfism, which had been present—unexpressed—in the same cells which also held the gene for tallness.

The Confluence of Classical Genetics with Cell Biology: The Genetic Role of Chromosomes

While Roux and Weismann argued at a biological level in favor of the chromosomes as the hereditary material, other investigators contributed chemical evidence. In 1881 the botanist E. Zacharias showed that Miescher's nuclein was located not indiscriminately in the nucleus but specifically in its chromosomes. On the basis of this fact, Hertwig and others who had recognized the nature of chromosomes as bearers of the hereditary material postulated that it is the nucleic acid moiety of the chromosomes which carries the genetic information. A proponent of this hypothesis was E. B. Wilson. In the first edition of his *The Cell in Development and Inheritance* (1896), one of the most scholarly books

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written by an American biologist, he summarized the findings which suggested that inheritance depends on a specific chemical substance in the chromosomes, the nucleic acid of Miescher. Wilson's synthesis undoubtedly was read widely, yet it remained without consequences: awareness of the existence of a substance in an organism obviously does not carry with it knowledge of its function. Altogether the observations and speculations which pointed first to the cell nucleus, then to its chromosomes, and further to their nucleic acid as the bearers of the hereditary material were inadequate to establish a complete "chromosome theory of inheritance." Such a theory demanded knowledge in two areas, cell biology and genetics, and the second of these did not yet exist.

When Mendelism came into being at the turn of the century there was waiting the accumulated evidence and speculations on the significance of the chromosomes in development and hereditary transmission. It took only a few years until the facts of hybridization of plants and animals and the microscopic studies of cells were coordinated (1903, 1904). In independent accounts by Wilson's American graduate student William B. Sutton and the German zoologist Theodor Boveri it was pointed out that the deduced type of transmission of Mendel's elements mirrored the observable type of transmission of chromosomes from one generation to the next. Sutton and Boveri reasoned as follows. Elements—that is, genes—occur in pairs; so do chromosomes. Members of each pair of genes segregate at the time of formation of germ cells so that each germ cell receives only one of the two genes. Chromosomes likewise occur in pairs and segregate at the time of germ cell formation, assigning only one of each pair to any egg or sperm. The members of different pairs of genes recombine at random at the time of segregation. Different chromosome pairs may well do the same: although observations at that time could not prove this inference to be true, they did not contradict it. The parallelisms of genetic and cytological facts found their explanation in the assumption that genes are located in the chromosomes—one of a pair of genes in one chromosome, the other in the partner chromosome. Different pairs of genes were assumed to lie in different pairs of chromosomes.

If this Sutton-Boveri theory of Mendelian inheritance was indeed not valid, one would have to discover other cellular particles that behave exactly like chromosomes. If it was valid, a tremendous advance had been made in the understanding of the basic organization of living systems. Instead of vague speculations about inheritance, whose prime rule was, it had been said, that there was no rule, the recognition of genes as separable entities and their placement in the chromosomes provided insight into the structural makeup of the self-replicating genetic material.

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The theory, however, did not conquer the minds of all geneticists. William Bateson, the foremost Mendelian in the early years of the century, continued to see his task as the abstract analysis of complex crosses. It led to the discovery of ever more gene pairs, without reference to their physical basis in the chromosomes. Others too were reluctant to see in the parallelism between the behavior of genes and chromosomes any more than a chance coincidence. What was needed was further, independent evidence for an intrinsic nature of the parallelism. The first fully convincing proof of the genetic importance of chromosomes came from Boveri in a preliminary report in 1902 and five years later in a volume of such exhaustive and penetrating critical ingenuity that it should be mandatory study for every biologist—were it not, alas, written in German.

Boveri was concerned with the results of abnormal fertilizations. When it had been established that fertilization normally involves the fusion of one egg nucleus with one sperm nucleus, it was soon seen that the occasional fertilizations of sea urchin eggs by two spermatozoa resulted in early death of the developing egg. Boveri set himself the task of finding the cause of the ill effect of double fertilization. He observed a great variability between different dispermic eggs in the stage at which they died and in the type of embryonic organ whose disintegration led to death. He also observed that within the same embryo or larva different quadrants developed in often strikingly different ways, one quadrant perhaps nearly normally, another very abnormally, and the remaining two quadrants in two different intermediately abnormal ways. In normal, monospermic sea urchin egg fertilization the sperm brings into the egg not only a nucleus but also a special organelle, the centrosome. This divides into two and forms a spindle between its two parts (Figure 1, A). There the chromosomes of egg and sperm nucleus arrange themselves in an equatorial plate, and after each chromosome has divided to form two sister chromosomes the spindle separates and assembles them in two sister nuclei. These come to lie in the two cells which are formed by cleavage of the original single egg cell.

The sea urchin species with which Boveri worked has 18 chromosomes in each of the fusing egg and sperm nuclei—that is, 36 chromosomes jointly. The longitudinal division of these 36 chromosomes results in the presence of 72 chromosomes, 36 of which go to one cleavage cell and 36 to the other. In dispermic fertilized eggs two centrosomes are introduced, which after division form four centrosomes. In many cases the four centrosomes come to lie in the egg like the four corners of a square, and four spindles are formed between them like swollen sides of the square (Figure 1, B). The first cleavage division of the egg results in four cells instead of the normal two. Each of the four ultimately forms

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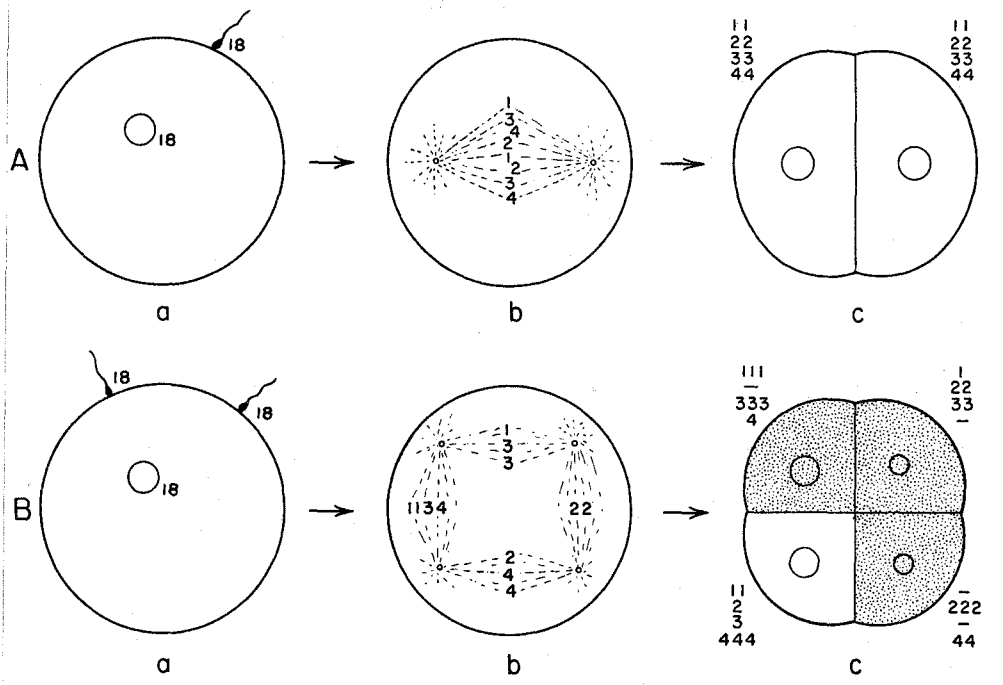

Figure 1.

A. (a) Normal fertilization of a sea urchin egg. The sperm head and the egg nucleus each carry 18 different chromosomes. Four pairs of the chromosomes, one of each pair from each parent, are shown in (b). They are designated 1, 2, 3, and 4.

(b) The first cleavage division with chromosomes 1, 1; 2, 2; 3, 3; and 4, 4. The chromosomes have not yet divided.

(c) After chromosome division and movement to the two poles of the division spindle, two cells are formed whose nuclei are identical in chromosomal content.

B. (a) Fertilization by two spermatozoa.

(b) Four cleavage spindles are formed with chromosomes 1, 1, 1; 2, 2, 2; 3, 3, 3; and 4, 4, 4. The chromosomes are irregularly distributed over the four spindles.

(c) Four cells are formed whose nuclei vary in size according to number of chromosomes as well as in kinds of chromosomes. The three stippled cells lack one or two kinds of chromosomes and have totals of 7, 5 and 5 chromosomes, respectively. The descendants of these cells die. The fourth cell, not stippled, has 7 chromosomes including at least one of each type. Its descendants will form a surviving quadrant of the embryo.

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one quadrant of the developing embryo or larva, just as the four cleavage cells formed by two successive divisions of a normally fertilized egg. The latter, however, form a normal larva; the former, a very abnormal one. In various ways Boveri showed that the difference in development cannot be attributed to double fertilization per se or to a number of other, secondary circumstances, but must be connected with the constitutions of the chromosomes. In a dispermic egg, egg and sperm nuclei together furnish $3 \times 18 = 54$ chromosomes at fertilization. These become distributed over four spindles. Observation showed that chance plays a large role with respect to which chromosomes go on which spindle. There is great variation from egg to egg; sometimes all spindles obtain approximately equal numbers of chromosomes, sometimes extremely unequal numbers.

The 54 chromosomes assembled on the four spindles split lengthwise thus result in 108 chromosomes. When the sister chromosomes move to opposite poles they are assembled in four nuclei. The size of these nuclei and their descendants in the four quadrants of the embryo depends on the number of chromosomes contained in them. By comparing the size of the diameters of the nuclei with the degree of developmental success in each quadrant of an embryo, as well as the developmental success in the quadrants of different embryos which have nuclei of similar size, Boveri came to the conclusion that variable numbers of chromosomes cannot account for the variability in development. Further considerations then made it clear that this variability was caused by qualitative differences between the nuclei.

When chromosomes were first investigated in such organisms as salamanders and onions they seemed all alike in size, and Weismann had speculated that they were alike too in genetic content. When Boveri studied the chromosomes of his sea urchins he was aware that they showed some size differences and that even more striking differences in size and shape had been found in other organisms. From there it was not a far cry to think of qualitative differences between the various chromosomes of a cell and to attribute the specific types of cellular degeneration of the embryonic quadrants to lack of specific chromosomes with their special qualitatively unique content. This theory could be tested in detail. According to it the 54 chromosomes of the just-fertilized dispermic egg should consist of three sets of 18 different chromosomes. Chance would assign any one chromosome to one of the four simultaneously formed spindles. One could calculate how often a resulting nucleus would fail to obtain all three of one of the 18 kinds of chromosomes, and then one could compare this theoretical expectation with the frequency of abnormal quadrants.

Boveri discussed the statistical problem with his colleague W. Wien in

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the physics department of Würzburg University. Wien, who knew that Boveri did not like even simple arithmetic computations, advised against an abstract treatment and instead proposed an experimental copying of the natural processes involved. Accordingly Boveri prepared a circular wooden tray on which he poured the well-mixed contents of a beaker —54 wooden spheres painted, in groups of three, with successive integers: 1, 1, 1; 2, 2, 2; . . . 18, 18, 18. He then placed onto the tray a wooden cross with arms the length of the diameter of the tray, thus separating the 54 spheres into four random groups that corresponded to the four chromosome groups present on the four spindles. In the living egg the chromosomes of each of the four groups would divide and the sister chromosomes would be distributed to opposite poles, thus forming four nuclei. In the model experiment the distribution of the chromosomes over the four nuclei could be determined from their distribution over the spindles. Whenever each "chromosome" was present in a nucleus at least once, the situation would lead to normal development. Whenever one or more "chromosomes" were not represented at all, the situation would lead to pathological development. Repeating the model procedure a hundred times yielded percentage estimates of expected normal and pathological development of quadrants which could be compared with actual results from dispermic eggs. The agreement between expectation and observation in this as well as in variants of the experiments was reasonably good, and the theory of qualitative differences between chromosomes became generally accepted. Worded differently, the theory stated that different chromosomes carry different genes.

Independent support for the theory came from consideration of a special chromosome found in grasshoppers and plant bugs. In 1891, in a descriptive study, the German investigator, H. Henking had found an unusual nuclear body of unknown function which he called X; this was present in one half of all sperm and absent in the other half. Eleven years later the American zoologist Clarence McClung wrote an article entitled "The Accessory Chromosome—Sex Determinant?" He had recognized the chromosomal nature of the odd body, and finding that all males of certain species had 11 pairs of ordinary chromosomes plus one "X chromosome," he proposed that this chromosome was the determiner of male sex. The suggestion that the X chromosome is sex-determining proved to be correct, although the specific conclusion that it was male-determining turned out to be wrong. Since his reasoning was based on an erroneously reported count of 22 chromosomes in females, McClung's assignment of male-determining properties to the accessory chromosome had been logical. But the female chromosome count should have been 24, consisting of 11 pairs of ordinary chromosomes and one pair of X chromosomes. It follows that the role of the X chromosome in the species

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concerned is female-determining: while one X is insufficient to direct development to femaleness, two X's do accomplish this goal.

McClung's fusion of the cytology of the X chromosome with the ancient riddle of sex determination was the first step in an analysis of the genic content of the X chromosome. Typical Mendelian experiments had shown that it made no difference whether a specific inherited trait was present in the mother or the father of a cross. Also, daughters and sons of such crosses were alike in either showing or not showing the trait. The chromosome theory of inheritance readily accounts for this "symmetry" in transmission, since most chromosomes occur in pairs, alike in females and males. The X chromosome, however, is an exception to the rule of alikeness: in many insects, as we have seen, and in many other species including man the X chromosomes occur in an "asymmetrical" fashion—twice in females and once in males. One could have predicted from this cytological situation that genes located in the X chromosomes would be transmitted asymmetrically.

Actually, not prediction but empirical facts of unexpected types of inheritance led to an interpretation in terms of genes in the X chromosome. The classical example of "sex-linked" inheritance is that of the white-eye trait in the fruit fly *Drosophila.* Crossing a red-eyed female with a white-eyed male produces daughters red-eyed like the father and sons white-eyed like the mother ("criss-cross" inheritance). This experiment was Thomas Hunt Morgan's, and his interpretation (in 1910) was the following. As a result of segregation of chromosomes the eggs produced by a female fly are all alike in having one X chromosome, but the sperm are of two kinds, with and without an X (the latter carrying a Y chromosome). X sperm fertilizing an X egg leads to development of a female; non-X sperm to that of a male. Every female, therefore, derives its two X chromosomes from both parents, one chromosome from each. Every male derives its single X chromosome from its mother. Now, if one assumes that the X chromosome carries one or the other of the contrasting genes for red (+) or white (w) eyes, one can symbolize the cross (1) red female by white male as $X^{+}X^{+} \times X^{w}$ and the reciprocal cross (2) white female by red male as $X^{w}X^{w} \times X^{+}$. The daughters of both crosses will be alike, $X^{+}X^{w}$ with the dominant w^{+} resulting in redness. The sons of the two crosses will be different, being X^{+}, red, in cross (1) and X^{w}, white, in cross (2).

The chromosomal interpretation of sex-linked inheritance in *Drosophila* was easily extended to man. Red-green colorblindness, hemophilia, and other traits whose transmission was known to follow certain rules with an obscure rational basis became the first examples of human genes to be assigned to a specific chromosome, the X.

The success of the chromosomal interpretation greatly strengthened

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the evidence for the chromosome theory in general. If the parallelism between the usual transmission of genes and the usual chromosomal situation could have been a chance coincidence, the additional parallelism between sex-linked inheritance and X-chromosomal behavior made a causal connection nearly obvious. Moreover, it was found that sex-linked inheritance occurred in two different ways—the *Drosophila* and man way, and the moth and chicken way. The results of genetic experiments with moths and chickens required that the females form two kinds of eggs, those with and those without an X chromosome, and that the males form only one kind of sperm, with an X chromosome. In other words, the females should be X and the males XX, which ran counter to the many findings on X chromosomes of various species in which the females always had been XX and the males X. No one, however, had thoroughly studied the chromosomes of moths and chickens. Cytological investigation was stimulated by the genetic experiments, and studies carried out by Jacob Seiler (1913) showed that the females indeed were X and the males XX. Here was a genetic exception from normal transmission of traits which was opposite in kind from the exception represented by white eyes in *Drosophila*. And the new type of exception found its parallel in a new kind of X-chromosome inheritance.

One more study in *Drosophila* provided the clinching evidence for the chromosome theory. If X-linked inheritance was an exception to the normal type, rare exceptions from X-linked inheritance were also encountered. Thus, in crosses of white-eyed females to red-eyed males an occasional daughter was white-eyed like the mother and an occasional son red-eyed like the father, contrary to typical criss-cross inheritance. Could the chromosome theory account for these exceptions from exceptions? It could. As Morgan's student Calvin Bridges reasoned in his doctoral thesis (1916), the exceptional white-eyed daughters must have received two X chromosomes from the mother instead of one, and the fertilizing sperm must have been of the non-X kind—that is, containing the Y chromosome. According to this interpretation the exceptional females must be of the sex-chromosomal constitution XXY. No such constitution had ever been seen. The exceptional red-eyed sons must have received no X chromosome from the mother, and the fertilizing sperm must have been the X-chromosome kind. Accordingly, the exceptional males must be of the constitution XO, where O stands for neither X nor Y—again a constitution which had never been seen. But when the sex-chromosomal constitutions of the exceptional offspring were studied under the microscope the cells of the females showed two X chromosomes and one Y chromosome and those of the males a single X chromosome and no Y. The prediction of cytological constitutions based on the aberrant offspring from crossing experiments seemed an achieve-

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ment of the human mind comparable to the prediction in 1846 of the existence of a new planet, Neptune, from deviations in the expected path of Uranus.

Chromosome Maps

It had been realized early that organisms have more genes than chromosomes. Each chromosome therefore must carry many genes. This is obvious for the X chromosome since many genes show X-linked inheritance, and it was also proven to be true for other chromosomes. A priori it was not known whether genes in the same chromosome always remain together or whether their physical "linkage" permits new combinations. Experiments proved the latter to be true. If in an individual one chromosome of a pair is marked by genes A^1 and B^1 and the other by A^2 and B^2, then his germ cells consist not only of the original classes A^1B^1 and A^2B^2 but also of two additional classes A^1B^2 and A^2B^1. In 1911 Morgan explained this phenomenon which he had encountered in his *Drosophila* crosses by assuming physical exchange of corresponding parts between the chromosomes of a pair during germ cell formation. He postulated that the genes in a chromosome are arranged in a linear sequence—prophecy of Roux!—and that each gene occupies a fixed locus in the sequence. If in an A^1B^1/A^2B^2 hybrid no exchange point falls in the interval between the loci of A and B, then the "noncrossover" combinations A^1B^1 and A^2B^2 are recovered among the offspring. If the two chromosomes break between the two loci and reunite in a crosswise way, then the "crossover" combinations A^1B^2 and A^2B^1 are produced. To these postulates Morgan added another. He had found that the frequency of crossovers among all combinations was different for different pairs of linked genes: for instance, in round numbers, 10 per cent for A-B, 18 per cent for A-C, and 1 per cent for C-D. These differences, the postulate went, were the consequences of differences in distance between the genes: C and D are located close together, A and B less so, and A and C least.

Morgan was not the first to observe linkage and recombination of linked genes. In England some years earlier, William Bateson and R. C. Punnett had discovered these phenomena in crosses of sweet peas. But the explanation which these geneticists proposed was abstractly formal and did not include reference to chromosomal events. Morgan's ingenious interpretation was partly suggested to him by a recently published paper (1909) of the Belgian cytologist F. A. Janssens, who described in fine detail the behavior of the chromosomes of a salamander during the development of its germ cells. This behavior suggested to Janssens that chromosomes may exchange sections between them. Mor-

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gan's "Attempt To Analyze the Constitution of the Chromosomes," as he entitled his "transforming" paper, was carried one step further by another of his great students. A. H. Sturtevant went beyond the general statement regarding genes lying "close together" or "more distant" from each other by using the numerical proportion of crossovers as an index for the distance between any two genes. "Then by determining the distances (in the above sense) between A and B and between B and C, one should be able to predict AC. For, if proportion of crossovers really represents distance, AC must be approximately, either AB plus BC, or AB minus BC" depending on whether the sequence of loci is A-B-C or A-C-B.[5] On the basis of these considerations Sturtevant, in 1913, constructed the first genetic map of a chromosome.

Experiments with many genes confirmed the prediction of the value of AC as either the sum or the difference of the values of AB and BC. This was taken as evidence for the reality of the linear arrangement of genes along the chromosomes. Later some geneticists emphasized that the additive and subtractive properties of linkage values could be interpreted in a variety of ways of which physically linear sequence was only one possible mode. Evidence accumulated, however, that provided proof of the Morgan-Sturtevant theory. One type of proof came from Harriet Creighton's and Barbara McClintock's study of the transmission of linked genes in corn concomitantly with that of the specific chromosomes involved in the linkage (1931). They used a pair of chromosomes whose partners differed visibly in some detail at each end: one chromosome had a "knob" at one end and was abnormal at the other end; the other chromosome had no knob at one and was normal at the other end. When the noncrossover combinations of genes located in the chromosome pair were studied microscopically the original doubly marked chromosomes were recovered, but when crossover combinations of genes were studied two visibly new types of chromosomes were seen: (1) one type having a knob at one end and being normal at the other, (2) the second having no knob and being abnormal. An equivalent proof of the chromosomal theory of crossing-over for *Drosophila* was published nearly simultaneously with that for corn.

Another type of proof for the theory of linear arrangement of genes in the chromosomes came from the analysis of the chromosomes in the salivary gland cells of *Drosophila* larvae.

In the 1920's when I was a postdoctoral Rockefeller Fellow working with Morgan, Bridges, and Sturtevant in the "Fly Room" at Columbia University, some of us playfully hoped that Santa Claus would bring us a microscope a hundred times more powerful than the best instruments then available to look at chromosomes. Of course, we knew from elementary physics that our hope could not be fulfilled (this was before the

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idea of an electron microscope had been conceived). Unknown to us, however, nature had kept in store an equivalent gift: in the nuclei of the salivary gland cells the chromosomes are a hundred times larger than the chromosomes in other cells, and they show a microscopically visible constant differentiation of thousands of bands specifically thick or thin, widely or closely spaced, analogous to the sequence of absorption bands in a spectrum. The salivary gland chromosomes are actually "cables" made up of thousands of closely held-together uncoiled individual strands, the products of successive divisions of originally single strands. This gives them their large diameter and makes visible their great length which in the uncoiled state is not clearly recognizable since a single strand is too narrow for recognition. When analysis of abnormal genetic situations led to deductions concerning abnormal chromosome constitutions the "salivary chromosomes" proclaimed the validity of the deductions. Thus, when it was postulated that some specific genes had been lost, this was mirrored in the loss of a section of bands or a single band of a chromosome. Likewise, when it was postulated that some genes had been translocated from their normal position to a new one, the salivary chromosomes showed an absence of a chromosome section at its usual place and its presence somewhere else. And when the genetic findings led to the hypothesis that a gene sequence had become inverted so as to produce the sequence A-D-C-B-E-F from the normal A-B-C-D-E-F, the salivary chromosomes showed the presence of the genic inversion by an inversion of a sequence of bands. The giant banded salivary structures had been seen decades before, but their true chromosomal nature was only established in 1933, by Emil Heitz and Hans Bauer. The correlations between positions of genes and order of bands in the chromosomes were demonstrated in the same year by Theophilus Painter, who profited from the advice of Wilson Stone.

The assignment of specific genes to specific chromosomes and the construction of chromosome maps by means of linkage data were the crowning achievements of age-old efforts to understand the morphological composition of the human body and other organisms. The first of the systematic attempts to gain such knowledge led to the development of the science of gross anatomy. When the skill of the anatomist became inadequate for the elucidation of the finer structures of the body, further advances were achieved by means of the microscope, methods of differential staining, and microchemical methods. Then the electron microscope made its appearance and revealed cellular structures close to and at the molecular level. Even such a powerful device of direct observation, however, would have been unable to lead to recognition of the genetic function of the structures observed or to the assignment of specific activities to specific structural elements. It required the subtle

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tools of the mind to specify the topography of the genes within the chromosomes, the microscopically visible assemblages of genes.

Morgan's dual role in the development of "higher Mendelism" is worthy of a comment. His own contributions in *Drosophila* genetics provided an understanding of sex-linked inheritance and laid the basis for the analysis of the genetic architecture of the chromosomes. After these accomplishments he served primarily as the synthesizer of genetic advances made by the members of his "school," particularly Sturtevant, Bridges, and H. J. Muller. The mutual influences which emanated from the division of labor between "the boss" and his collaborators were strengthened by a climate of equality created by Morgan. How rare to find a mature, already famous biologist inviting his students, who hardly had earned their doctoral degrees, to be coauthors with him of the fundamental textbook of the new genetics—*The Mechanism of Mendelian Heredity* (1915)!

For Morgan himself the *Drosophila* work was only one aspect of a biologist's searching. Morgan always had a multitude of projects going simultaneously: some promising, others foolish, he said, and still others so foolish that he did not want to talk about them. He was a prominent experimental embryologist, a lifelong student of regeneration, of sex determination, of fertility and sterility, of the hormonal effects of castration in chickens, of the genetics of mice, rats, and pigeons, of evolution. For a biographical memoir of Morgan, Sturtevant made a list of over fifty kinds of animals and one plant on which Morgan had published results of his work.[6]

Even at the height of his achievements during the first years of the *Drosophila* period he experimented with other forms and wrote extensive accounts of this work. Sturtevant characterizes Morgan's strengths as "involving great skill and patience in the collecting and care of animals, insight in seeing what were the critical points to study, and ability to recognize and to follow up unexpected facts."[7] No wonder that Morgan made many important discoveries and also that he soon shifted his emphasis from the *Drosophila* work. Notwithstanding, for more than one and a half decades he remained the synthesizer of many of the findings of the "Morgan school." Gradually, however, he was left behind. I remember the "awe-full" moment when Bridges explained to him a particularly intricate new result and the initiator of it all left the room, shaking his head and saying "too much for me!"

Physical Nature of the Gene

Although the chromosome theory had lifted the concept of the gene from a solely operational unit to a material entity of known localization,

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it had not brought clarification of the physical nature of the gene. Chromosomes are made up of nucleoproteins—macromolecules of nucleic acids joined to protein macromolecules—and the question remained, What part of the chromosomes represent the genetic material? On the basis of inadequate information on the chemistry of nucleic acids it seemed unlikely that these molecules occur in a sufficiently large number of variants to equal the very large number of different genes.

Moreover, cytological observations seemed to show that chromosomes may at certain stages lose their nucleic acid component. E. B. Wilson, who in the first edition of his earlier-cited book on the cell (1896) had regarded nucleic acid as the hereditary substance of the chromosomes, had abandoned this viewpoint when the third edition appeared in 1925. In a section entitled "Staining-Reactions of the Cell-Substance" he wrote: "So far as the staining reactions show, therefore, it is not the basophilic element (nucleic acid) that persists, but the socalled 'achromatic' or oxyphilic substance. The nucleic acid component comes and goes in different phases of cell activity and it is the oxyphilic component that seems to form the essential structural basis of the nuclear organization."[8]

Attention was focused therefore on the "oxyphilic" protein moiety of the chromosomes. Proteins were known to be of innumerable kinds, functioning in many different ways; those that were enzymes were effective even when present in very small amounts. For these reasons genes were usually believed to be proteins, and the nucleic acid molecules were regarded as perhaps skeletal rather than genetic elements of the chromosomes.

Lacking exact information on the genic nature, how should the geneticist define a gene minimally? H. J. Muller (1890–1967) revolved this question in his mind all during his long, active life and emphasized two aspects of genes: their ability to replicate themselves accurately and their ability, if changed, to replicate themselves in the changed form. Genes, of course, do not replicate without a cellular machinery that provides the material and the energy for building two genes where only one is present to begin with. This machine itself, however, is not self-replicating; its existence and multiplication depend on genes. If one removes some of the nonnuclear material from a cell, it is able to regenerate fully the normal amount. If one removes a gene, no regeneration occurs. A gene then is a cellular unit which arises only from a preexisting gene. This minimum definition is clearly not sufficient to lead to decisions concerning the chemical nature of a gene.

Using ultraviolet radiation and exploring the mutagenic efficiency of different wavelengths of the ultraviolet spectrum, Lewis Stadler, Alexander Hollander, and others had attempted to characterize the class of compounds to which genes would belong. This work, begun in the middle

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thirties, gradually bared a close relation between the absorption spectrum of nucleic acid and the mutagenic effectiveness of the components of the spectrum. The investigators were on the right track, but various complications prevented definitive conclusions.

Then, chemical knowledge came from an unexpected source. In 1928 the British physician F. Griffith worked with rabbits which had been infected with a harmless strain of the bacterium pneumococcus. He also had a virulent strain of the organism. Nothing happened when he injected killed bacteria of the virulent type into noninfected rabbits. But when he injected the harmlessly infected rabbits with killed bacteria of the virulent type the nonvirulent pneumococci changed to virulence and the rabbits died. O. T. Avery at the Rockefeller Institute for Medical Research set himself the task of discovering what part of the killed bacterial material was essential in the transformation of avirulent into virulent microorganisms. In 1944, working together with M. McCarty and C. M. MacLeod, he had the answer. It was DNA. In a letter of May 13, 1943, to his brother the 66-year-old Avery wrote: "The . . . material . . . conforms *very* closely to the theoretical values of pure desoxyribose nucleic acid . . . (Who could have guessed it) . . . If we are right . . . then it means that nucleic acids are not merely structurally important but functionally active substances in determining the biochemical activities and specific characteristics of cells."[9] It was to be nearly a decade before the double helix model for the structural composition of DNA was worked out by James Watson and Francis Crick. It provided the beginning of an answer to the problem of self-replication of an original DNA gene as well as of changed forms of it.

Having stressed the chromosome theory of inheritance, it is necessary to raise the question of whether some part of inheritance must be assignable to extranuclear, nonchromosomal elements. Presumably such elements should be located in the cytoplasm of cells and would be expected to be transmitted in a non-Mendelian manner. There was a period in which the proponents of cytoplasmic transmission of certain traits regarded themselves as superior to the proponents of chromosomally located genes, an attitude which had its counterpart in an aggressive defense reaction by the latter proponents. In the course of time the two camps modified their divergent opinions and agreed that most traits follow Mendelian rules and are dependent on nuclear genes. A few traits, however, such as some inherited variations of chlorophyll bodies in plants or of respiratory enzymes in yeast cells, are transmitted by self-replicating elements in the cytoplasm—cytoplasmic genes. During the last few years it has been found that these genes are molecules of DNA just as the nuclear genes.

In 1927, Muller had succeeded in experimentally causing "artificial

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transmutation of the gene" in *Drosophila,* and the same was soon accomplished by Stadler in plants. The application of X rays and other ionizing radiation to these organisms caused genes to mutate. How the energy-rich radiation changed the gene molecules remained obscure—not surprisingly in view of the lack of knowledge of genic structure. During the Second World War new mutagens were discovered: certain chemicals such as mustard gas. Disappointingly, neither radiation nor chemical mutagenesis was specific in singling out individual genes for change. By and large, mutations were produced at random among the thousands of possible targets. Nevertheless, the impact of these discoveries was great: it was the first time in history that man intentionally could alter the hereditary makeup of individuals by changing the genetic material itself. All earlier manipulations of genes were of a type in which selection or chance sorted out some already existing genes for survival and others for rejection.

Specifically, as Muller recognized from the very beginning, artificial mutations were inducible in man as well. Muller warned of the ill consequences for future generations when human ovaries and testes are irradiated for diagnostic and therapeutic reasons and when mutations thus induced in the germ line are transmitted to the offspring. A fortiori such concerns have deepened ever since the genetic effects of nuclear explosions, radioactive fallout, and contamination have disturbed the thoughts of a whole generation of biologists and of mankind in general. More recently the possibility of chemical mutagenesis by man-made agents in the environment or by individual use of such agents has become a cause of warnings and of calls for intense specific research. It is implied in the foregoing that mutations are harmful to their bearers; although this is not the case for all mutations, it is so for many.

Genes have been recognized not by direct study of their structure but by their effects. Usually these effects were far removed from the primary actions of genes, actions which until recently were not accessible to observation. A wide gap lies between a chromosomal gene for seed shape and its visible effect on the seed, or between a gene for body size and the actual stature attained. It was only slowly that these gaps were bridged by tracing backwards as far as possible the development of the end effect. For a long time geneticists were primarily occupied with the Mendelian type of separation of the genic content of organisms into its components and with the exhilarating successes of gene localization. Thus it came about that an early fundamental foray into the problem of genic action remained isolated and without significant influence.

It was the English physician A. E. Garrod who early in the present century studied certain biochemical abnormalities in human individuals, found that they were due to Mendelian genes (then newly rediscov-

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ered), and traced in detail the biochemistry of what he termed "inborn errors of metabolism."[10] He recognized that these consisted in the inability of the organism to perform a step in a sequence of chemical reactions. Normally, specific enzymes permit the various steps to be made; the errors of metabolism depend on the absence of a normal enzyme, resulting in an interruption (a "block") at some specific point in the sequence of reactions. This basically new insight into genic action became well known to biochemists but had no deeper influence on the thoughts of geneticists until thirty years later.

Only when the first urgent problems of genetics had been solved did interest return to the mode by which genes exert their effects. Large-scale experiments by George W. Beadle and Edward L. Tatum on the bread mold *Neurospora* furnished an abundance of "biochemical mutations," and their effects were traced to specific blocks in the multidimensional network of reactions. As in man these blocks were traceable to deficiencies of specific enzymes, and a general hypothesis of genic action was summarized in the phrase "one gene–one enzyme," meaning that genic action consists in the production of enzymes and that each gene is responsible for a different enzyme. This fruitful hypothesis later became further generalized under the impact of the molecular genetics of the reactions controlled by the DNA genes and the unraveling of the structure of proteins. "One gene–one polypeptide chain" encompassed dependence of enzymes on genes as well as of other types of molecules such as hemoglobins. Genetics had become an integral aspect of biochemistry and cell physiology.

The Influence of Genetics on the Biological Sciences

Genic action par excellence is involved in the differentiation of the fertilized egg during development. Historically, embryology had first been dealt with as a purely observational discipline, then an experimental science. Many of the insights into the processes that determine the fate of different parts of the developing whole had been obtained at a level which did not require references to specific genes. Yet it had long been obvious that differentiation is the result of differential gene activity in various parts of the embryo. There seemed to be a paradox between the knowledge that mitosis furnishes all cells of an embryo with the same genic-chromosomal content and the evidence for inequality of differentiation in different cells. Weismann had tried to overcome the paradox by denying equality of gene content and postulating instead hereditarily unequal cell divisions during development. This artificial postulate never carried conviction, and during the 1890's other biologists such as O. Hertwig, Wilson, and Hans Driesch developed theories of differentiation

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according to which all genes are present in all cells, with some genes being called into action in some cells and other genes in other cells. These theories remained mainly at a general level, but here and there evidence became available for effects of specific genes on specific types of differentiation. (An example is furnished by a gene in *Drosophila* which directs the differentiation of certain cells to become sense organs. In the absence of the gene the same cells differentiate into skin cells.) How genes become turned on and off during development is now viewed in light of findings on the molecular regulation of gene activity in microorganisms, where regulator and operator genes, affected by extragenic variables, determine the functioning of other genes.

In still another biological area, that of evolution, genetics has played an outstanding role. It has redirected the emphasis of evolutionists from consideration of the individuals of a species to the abstract permanent gene pool of the population, whose mortal recombinants arise from it. Evolutionary genetics has thrown light on the permanence of species as well as on their changes, has studied differences between species in individual genes as well as in whole chromosomes. This is not the place to enter into discussions about experimental and theoretical population genetics as applied to evolutionary processes. Neither can we deal with evolutionary cytogenetics—the correlated study of cells under the microscope and of genes in hybridization experiments in their joint bearing on evolution.

One evolutionary event should be detailed, though, since it deserves much wider recognition than is usually awarded to it. This is the fact that a "good," that is, well-defined, plant species of the mint family, the hemp nettle *Galeopsis Tetrahit*—assigned specific rank by no less a systematist than Linnaeus—has been recreated out of other good species in the experimental plots of the Swedish geneticist A. Müntzing. Furthermore, several other new forms have been similarly produced which fulfill the designation species. The experiments consisted of hybridization between distant natural species. Species hybrids are usually sterile, because the chromosomes of the two parental forms do not pair well and as a result the ovules and pollen grains of the hybrids obtain abnormal combinations of the parental chromosomes. Exploitation of rare chromosomal processes, however, may lead to the doubling of all chromosomes in a hybrid, which will enable the doubled chromosomes to pair with each other and will therefore lead to fertile ovules and pollen. In different organisms evolution proceeds in different ways, and the examples of sudden "cataclysmic" origin of new species are typical for only a fraction of evolutionary events. Nevertheless, it is important to stress that the origin of species has been observed directly, and not only deduced theoretically.

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Genetics as Applied to Man

Genetics as a science encompassing all living systems has been constructed out of discoveries in peas, flies, mice, fungi, bacteria, and viruses. Each kind of organism added to the generality of genetic findings at the same time that it demonstrated specific and often striking variations in the form in which the general principles were expressed. Man is an organism which physically represents a typical animal species but has psychological and sociological attributes setting him apart from all other forms. Human genetics at the physical level has shown man to be subject to the same genetic regularities as other animals. Who would have not expected it to be so? On the other hand, discussions concerning genetic determinants of man's mental characteristics and of his cultural achievements have been a battleground between extremists: those who believed in almost complete genetic determinism and those who believed in the complete nongenetic malleability of the mind and the potentially ubiquitous realization of its highest cultural achievements. Dogmatism on both sides has led to estrangement between camps which would have profited from collaboration, but the ideological warfare has also had its constructive aspects. For every finding that made one side proclaim "never underestimate the power of genes," the other side adduced a finding that justified its warning "never underestimate the power of the environment." The education of each camp by the other has made progress, and the number of die-hard extremists has greatly declined. Genetics will not cease to remain a central issue in uniquely human concerns: the preservation of the genetic endowment of mankind, its improvement by selective processes, the future application of biological methods to change genes at will, the not-so-utopian possibility of perpetuating "desirable" gene combinations in successive asexually reproduced generations, the problems of the genetics of race and of socio-economic stratifications, to name some of them.

Genetics, as applied to man, is a vulnerable science. Its ultimate usefulness in concepts and in practice is matched by its potential misuse. Such misuse has played a role throughout the twentieth century in the rash conclusions and popular exploitation of the opinions of naïve eugenicists and in their tragic consequences in Hitler's Germany. Another example of the vulnerability of genetics was the period of the dilettante movement of Lysenkoism in the Soviet Union and its physical and spiritual satellite countries (which on a dogmatic level also included China).

Conclusion

The period of genetics which has been surveyed in this essay clearly belongs to "little science." Mendel's small experimental plot in the garden

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of his monastery, Morgan's crowded Fly Room at Columbia University, and even Muller's thousands of glass vials in which he analyzed the mutagenic effect of X-rays all involved limited enterprises, in an external sense. When Bridges and Sturtevant had earned their doctoral degrees in 1915, Morgan obtained a grant from the Carnegie Institution of Washington to pay for their salaries as research assistants and also for the maintenance of living stocks of *Drosophila*. It would be of interest to know how large—or rather how small—the annual budget was.

The little-science attributes of the Morgan school extended to the physical state of the Fly Room itself. When rain fell on the roof of Schermerhorn Hall, a bucket had to be placed in the room to catch the water which leaked through the ceiling. The "microscope lamps" were ordinary light bulbs, shaded by metal sheets cut out of ordinary cans. The room abounded with fruit flies either escaped from milk bottles which served as culture vessels or permanently bred in the never thoroughly cleaned garbage can of the room. In addition there was other life. During the two years I worked in the inspiring spirtual atmosphere of the Fly Room I never opened my desk drawer without looking away for a while to give the cockroaches a chance to run into the darkness. Once I said breathlessly, "Dr. Morgan, if you put your foot down you'll kill a mouse." He did! Some years later, Morgan founded a well-equipped modern laboratory at the California Institute of Technology, but during the Columbia years little science was the appropriate way to make scientific progress instead of assigning time to technical perfection.

The continuity of genetics rests on internal necessity. So many pieces in the storehouse of biological facts fit together that it was unavoidable to recognize the unifying power of the discovery of the gene. In detail, however, instead of continuity there have been starts and all-too-early stops, witness Mendel and Miescher and Garrod.

The way in which genetics developed from the confluence of separate currents was hardly the only possible one. Historical accident played a large role. One can speculate that basically the transformation of the pneumococcus microbe from avirulence to virulence could have been discovered in 1870 instead of six to eight decades later. It would have led very early to identification of the genetic material with DNA. Extensions of the transformation work to other genic properties of the pneumococcus and the discovery of linkage in simultaneous transformation of several genic properties could have led to the construction of a genetic map long before Sturtevant's feat in *Drosophila*. Or, one may speculate what would have happened if chromosomes and the essentials of their behavior had not been discovered in the 1880's but fifty years later. Mendelian analysis from 1900 to 1930 would have remained

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without the clarification in form of the chromosome theory which actually came early. One may speculate even more brazenly. Could not one of the pioneering agriculturalists in the Middle East have counted the numbers of different plants in a segregating field? Could he have observed 3:1 ratios (perhaps helped in his arithmetic by belonging to a people who used the duodecimal rather than the decimal system)? Could his or his followers' interpretation of what we now call Mendelian ratios have led to the discovery of the binomial theorem millennia before Newton?

These are idle questions, but I raise them to confess my belief in a reality of the external world however inexhaustible in character and however incompletely represented at any stage of its recognition. Given enough time, genetics, whether born in ancient history or in the most recent centuries, would have reached a state very similar to the present. Whatever the paths taken during the rise of genetics, the purification of the concept of the gene would have been the final result. The gene at first was regarded as a *living* unit, a submicroorganism. This concept had to yield to the recognition that the gene as a molecular species is the basis of life in living *systems*, organisms. With this recognition more modest demands were made in assigning functions to the genes, and simultaneously more penetrating analyses of these functions became possible.

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GUNTHER S. STENT

DNA

The Golden Jubilee

In September of 1950 the Genetics Society of America held a symposium at Ohio State University to celebrate the fiftieth anniversary of the rediscovery of the work of Gregor Mendel, founder of the scientific study of heredity. The proceedings of that symposium were published under the title *Genetics in the 20th Century*, in the form of twenty-six essays written by some of the most eminent geneticists of the time.[1] In his introduction to these essays the editor, L. C. Dunn, of Columbia University, writes that their "primary purpose was to survey the progress of the first fifty years of genetics and to exemplify the status of some of its problems today." These problems, Dunn points out, extended beyond the confines of pure and applied biological science to the arena of the then ascendant Cold War: "no one in 1950 can be unaware of the fact that the principles upon which genetics rest have been declared politically unacceptable in Russia and that the other communist countries generally have followed suit." However, its alleged conflict with the tenets of dialectical materialism notwithstanding, "genetics has become a many-sided body of knowledge and method dealing with questions which are recognized as of central importance in all efforts to understand living matter—how it perpetuates itself through reproduction, how it changes and adapts itself to its environment. Many of its principles have turned out to have a general character, so that not only do the rules apply to plants, as Mendel first found, but to animals of all kinds, to man himself, and to the whole world of microorganisms, bacteria and viruses, revealed since Mendel's time." Finally, Dunn observes that "in spite of its evident diversification, genetics has fortunately retained the essential unity given to it by [Mendel's] discovery of a fundamental element of heredity, the gene, so that varied problems can be stated in a common language which is becoming more generally understood."

This essay is dedicated in grateful affection to my teacher André Lwoff.

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The achievements which the distinguished essayists celebrated form a body of knowledge that is nowadays generally referred to as *classical genetics*. And it is precisely in the gene concept, which had provided its common language (and which had given such offense to primitive ideologues such as Trofim Lysenko), that classical genetics is to be differentiated from the *molecular genetics* that was to follow in its wake. The fundamental unit of classical genetics is an indivisible and abstract gene. In contrast, the fundamental unit of molecular genetics is a concrete chemical molecule, the nucleotide, with the gene being relegated to the role of a secondary unit aggregate comprising hundreds or thousands of such nucleotides.*

As perusal of the Golden Jubilee essays shows, the gene concept had remained largely devoid of any material content for the fifty years following the rediscovery of Mendel's work. Besides not having fathomed its physical nature, classical geneticists had been unable to explain how the gene manages to preside over specific cellular physiological processes from its nuclear throne, or how it manages to achieve its own faithful replication in the cellular reproductive cycle. Herman J. Muller of Indiana University, then one of the elder statesmen of genetics, epitomizes that condition of classical genetics in his essay "The Development of the Gene Theory" in these terms: "the real core of genetic theory still appears to lie in the deep unknown. That is, we have as yet no actual knowledge of the mechanism underlying that unique property which makes a gene a gene—its ability to cause the synthesis of another structure like itself, in which even the mutations of the original gene are

* The draft of this paper referred to the classical gene as being not only an indivisible and abstract unit but also a *transcendental* unit. In the Bellagio conference discussion my use of all three of these adjectives was criticized. So far as "transcendental" is concerned, I have now eliminated it, as a possible source of confusion, even though I still think that its common (rather than Kantian) meaning, namely possessing attributes so fantastic as to be beyond ordinary comprehension, *is* applicable to the classical gene. As far as the other two adjectives are concerned, Curt Stern expressed the view that the gene was "indivisible" only because geneticists did not *know* how to divide it, and that it was not really "abstract" because geneticists were perfectly aware that it had a material basis, genes having in fact been shown to reside on chromosomes. To me it appears, however, that both indivisibility and abstractness of the classical gene were fundamental epistemological qualities, which took their origin in the kind of operations by means of which the gene was then studied. From the classical purview, it would have been *meaningless* to divide the gene and *futile* to endow it with a concrete physical identity. For instance, I remember that in 1949 there appeared in *Life* magazine an electron micrograph of an ultrathin chromosome section, which purported to show the first picture of a gene. The patent speciousness of this claim at the time illustrates the basic cognitive difficulty that existed as recently as twenty years ago: how could one recognize a gene as a gene even if one happened to lay eyes on it?

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copied . . . What must happen is that just that precise reaction is *selectively* caused to occur, out of a virtually infinite series of possible reactions, whereby materials taken from a common medium become synthesized into a pattern just like that of the structure which itself guides the reaction. We do not know of such things yet in chemistry." But for the classical geneticist study of the detailed nature and physical identity of the gene, though undoubtedly of great intellectual interest, is not really an essential part of his work. His theories on the mechanics of heredity and the experimental predictions to which these theories lead are largely formal, and their success does not depend on the knowledge of structures at the submicroscopic, or molecular, level where the genes lie. Application of the adjective "classical" to that first phase of genetic research is rather analogous to its well-established use in "classical physics." The fundamental and indivisible conceptual unit of nineteenth-century classical physics was the atom, which despite its unfathomed nature, had allowed very far-reaching insights into the macroscopic properties of matter. "Atomic" twentieth-century physics later succeeded in explaining the nature of the atom in terms of subatomic phenomena, just as molecular genetics was to succeed in explaining the molecular nature of the gene in terms of subgenic phenomena.*

One Gene-One Enzyme

Among the twenty-six Golden Jubilee essays there are only three which deal to any extent with matters that were of immediate relevance for the then-nascent molecular genetics. One of these essays, "Genetic Studies With Bacteria," was written by Joshua Lederberg, at the time a twenty-five-year-old professor-prodigy in the University of Wisconsin. Lederberg reviews his recent work on bacterial sexuality, which he and Edward L. Tatum had discovered just four years earlier.[2] His discussion of bacterial genetics is still entirely "classical," in that the

* At the Bellagio conference Erik Erikson wondered what is actually meant by "classical." He received a variety of responses to his query, such as that "classical" means "of no concern to me," and hence "embalmed"; "what you learn in school"; "perfect in content and form"; "simple and perfect in form"; and that it pinpoints dogmas from which young people are encouraged to make heretical departures. Léon Rosenfeld stated that for Bohr "classical physics" meant perfection in physical description, and that the later atomic physics is "nonclassical" in the sense that the language of everyday macroscopic experience can give only an imperfect description of the microscopic world of atoms. Dr. Rosenfeld's remark makes the correspondence between classical physics and classical genetics even closer than I had previously thought. For the macroscopic character differences on which the concept of the Mendelian gene was based are likewise closer to our everyday experience than the microscopic nucleotide unit of molecular genetics.

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gene is treated as the fundamental unit responsible for the phenomena under study. Indeed, Lederberg takes some pains to make clear that the genetic mechanisms of the lowly bacteria can be understood from the classical viewpoint developed through study of higher forms. His essay pertains to molecular genetics only insofar as it describes what was to become one of the chief "molecular" experimental systems, the sexually fertile bacterium *Escherichia coli* K12 isolated at Stanford University "in the fall of 1922 from the stools of a diphtheria convalescent."

George W. Beadle, then Thomas Hunt Morgan's successor as chairman of the Biology Division at the California Institute of Technology, contributed an essay entitled "Chemical Genetics," which traces the development of the concept that genes preside over cellular function by controlling the chemical reactions of cell metabolism. Beadle describes how in the 1930's he and Boris Ephrussi (who also contributed an essay, concerned mainly with extranuclear inheritance) worked on the genetic control of the formation of the eye color of the *Drosophila* fruit fly and how he finally became discouraged over the difficulties encountered with that material. In 1940 he and E. L. Tatum turned their attention to a more favorable organism, the bread mold *Neurospora*. According to Beadle: "With the new organism our approach could be basically different. Through control of the constituents of the culture medium we could search for mutations in genes concerned with the synthesis of already known chemical substances of biological importance. We soon found ourselves with so many mutant strains unable to synthesize vitamins, amino acids and other essential components of protoplasm that we could not decide which ones to work on first." Beadle reports that in the intervening ten years he, Tatum, and their collaborators managed to analyze the genetic and biochemical characteristics of enough *Neurospora* mutants to lend strong support for the "one-gene–one-enzyme" hypothesis.[3] (In his essay Beadle actually prefers the name "one-gene–one-function" hypothesis, but it was the former name which became popular in the community of geneticists.) This hypothesis states that each gene has only one primary function, which in most or all cases is to direct the synthesis of one and only one enzyme, and thus to control one single chemical reaction catalyzed by that one enzyme. Though (as Beadle emphasizes) the idea that genes control single functions was not really original with him or Tatum, there can be little doubt that their clear formulation and strong experimental evidence for that hypothesis had a profound impact on subsequent thought about the gene. It must be noted, however, that the gene of Beadle's essay is still the classical, indivisible, abstract unit. But in promulgating the belief that each gene is doing only one thing, the one-gene–one-enzyme hypothesis gave hope of ultimately being able to find out how that thing is done.

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Nucleic Acid

The third essay touching on molecular genetics was one by Alfred E. Mirsky of the Rockefeller Institute for Medical Research, entitled "Some Chemical Aspects of the Cell Nucleus." Chemical study of the cell nucleus was begun in the 1860's by Mendel's contemporary, the Swiss chemist Friedrich Miescher,[4] before the notion had emerged that the cell nucleus is the seat of heredity. Miescher undertook an analysis of cells, such as pus cells and salmon sperm, in which he knew the nucleus to represent a large fraction of the total cell mass. These analyses revealed that the nucleus contains a hitherto unknown, phosphorous-rich, acid substance, to which a later worker gave the name *nucleic acid.*

By the turn of the century the ubiquitous presence of nucleic acid in plant and animal cells had been demonstrated, and the German biochemist Albrecht Kossel had identified the nucleic acid building blocks (Figure 1): the four nitrogenous bases *adenine, guanine, cytosine,* and *uracil* (the former two belonging to the class *purines* and the latter two to the class *pyrimidines*), a five-carbon sugar, and phosphoric acid. Further analytical work, largely by P. A. Levene[5] and by W. Jones in the 1920's, showed that there exist two fundamentally different kinds of nucleic acid, which are now called *ribonucleic acid,* or *RNA,* and *deoxyribonucleic acid,* or *DNA.* RNA contains *ribose,* whereas DNA contains *deoxyribose* as its five-carbon sugar. DNA, furthermore, does not contain the pyrimidine uracil; instead of uracil it contains 5-methyl uracil, or thymine. Nitrogenous base, sugar, and phosphoric acid were found to be linked to form a *nucleotide.* By the 1930's it had been shown that nucleic acid molecules contain several such nucleotides linked through phosphate diester bonds between their ribose or deoxyribose sugar molecules (Figure 1). Another ten years had to elapse before the enormously high chain length of nucleic acids was finally appreciated. As we now know, nucleic acid molecules represent *polynucleotide* chains of thousands, and sometimes millions of nucleotides in continuous chemical linkage.

Mirsky's essay is concerned mainly with the biological significance of the DNA type of nucleic acid. It sets forth how the invention in 1924 by R. Feulgen[6] of a specific color stain for DNA made it possible to show that the cell DNA is located almost exclusively in the *chromosomes* of the nucleus, whereas the cell RNA, despite its being a "nucleic acid," is located mainly in the cytoplasm. Since the chromosomes are the cell organelles in which the genes were known to reside, it did not seem far-fetched to imagine that DNA plays some important role in hereditary processes. But as the chromosomes contain even more protein than DNA,

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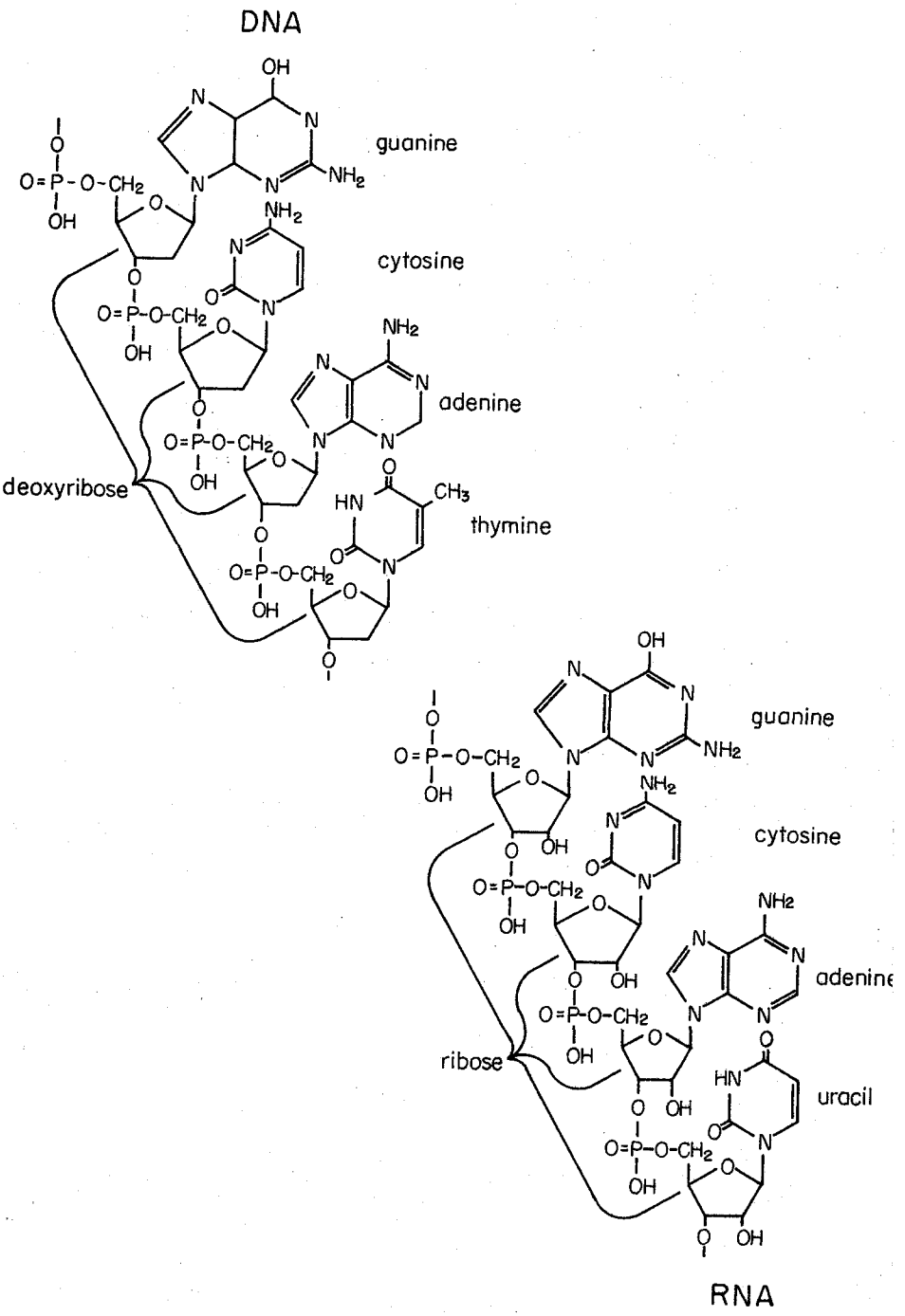

Figure 1. The polynucleotide chains of deoxyribonucleic acid, or DNA, and of ribonucleic acid, or RNA.

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it was not necessary to infer that the genes actually contain any DNA. In support of the view that "DNA is part of the gene substance," however, Mirsky cites his own observations that "in the different cells of an organism the quantity of DNA for each haploid set of chromosomes is constant . . . constancy per cell is certainly an unusual characteristic for chemical components of cells . . . Even a substance, such as RNA, present in all cells, varies greatly in amount in different cells."

As far as the chemistry of DNA is concerned, Mirsky announces the recent demise of the *tetranucleotide theory*, "according to which a polynucleotide containing one of each of the four bases was considered to be a fundamental unit in DNA. Using chromatographic procedures for analysis [Erwin] Chargaff and his colleagues have shown that the four bases [adenine, guanine, cytosine, and thymine] are not present in equimolar proportions [as had been previously believed], and this has removed whatever experimental basis there was for the tetranucleotide theory." Furthermore, "Chargaff and his colleagues have analyzed the DNA's prepared from a number of different sources and have obtained results showing that in preparations from different organisms the ratios of the bases are different, although they are the same in preparations from different tissues of the same organism. It is highly probable, therefore, that the proportion of the four bases differ in DNA's of various organisms."

Mirsky does not, however, mention another seemingly less important finding which Chargaff presented in the very paper[7] cited in the essay. This finding is the DNA compositional equivalence rule, which states that although the proportion of the four bases differs in the DNA's of various organisms, it is nevertheless true that the molar proportion of adenine is always equal to that of thymine and the molar proportion of guanine is always equal to that of cytosine. Three years later, this rule was to provide a crucial clue for divining the structure of DNA.

Transforming Principle

In 1944, or six years before the Golden Jubilee, work had been published (and was certainly well known to most of the essayists) which proved that DNA is not only "part of the gene substance" but *is* the gene substance. Yet Mirsky's essay is the only one of the twenty-six in which the implications of that work are discussed. (Lederberg's Golden Jubilee essay refers to it briefly as a promising development in bacterial genetics, and Beadle's essay devotes two sentences to it, saying that it "has certainly introduced another chapter in genetics, and one that promises to be among the most exciting. It has given chemists new

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incentive to learn about the nucleic acids, compounds which everyone recognizes to be extremely important biologically and about which so little is known.")

This work had been carried out by Oswald T. Avery[8] and his collaborators at the Rockefeller Institute and represented the identification of the active principle of *bacterial transformation,* a phenomenon first observed in 1928 by the British bacteriologist F. Griffith.[9] Avery could show that upon addition of purified DNA extracted from normal *donor* bacteria to mutant *recipient* bacteria, which differ from the donor bacteria in one mutant gene, some of the recipient bacteria are transformed hereditarily into the donor type. Thus the normal donor gene must have entered the transformed recipient bacterium in the form of a donor DNA molecule and there displaced its homologous mutant gene. Hence it followed that the bacterial DNA represents the bacterial genes. In 1944 this conclusion seemed so radical that even Avery himself was reluctant to accept it until he had buttressed his experiments with the most rigorous controls.

Avery's controls were evidently not rigorous enough for most contemporary geneticists, including his Rockefeller Institute colleague Mirsky, who was of the opinion that "it is quite possible that DNA, and nothing else, is responsible for the transforming activity, but this has not been demonstrated conclusively. In purification of the active principle more and more of the protein attached to DNA is removed . . . It is difficult to eliminate the possibility that the minute quantities of protein that probably remain attached to DNA, though undetectable by the tests applied, are necessary for activity." But Mirsky concedes that "it can be regarded as established that DNA is at least part of the active [transforming] principle." As we shall see presently, Avery's work, just as Mendel's, had been too far ahead of its time, so that for some eight years it had very little impact on genetic research. Although, in contrast to Mendel, Avery and his discovery were well known, the genetic role of DNA had to be rediscovered in 1952 through work with bacteriophages.*

* In the Bellagio conference discussion Charles Weiner stated that he "always recognizes it as a danger signal when someone says 'so and so was too far ahead of his time,' a discovery or a man. This is a method of obscuring what went on . . . to say that it was ahead of its time is, essentially, to take a shortcut through history." Of course, pronouncing a discovery to have been ahead of its time just *because* it was not immediately appreciated is to make an empty tautology. But I am appealing here to another criterion of premature discovery that ought to illuminate rather than obscure what went on. By this criterion a discovery is "ahead of its time" if the inferences to which it leads cannot be connected by a series of simple logical steps with contemporary canonical knowledge. Thus, Curt Stern's essay shows that Mendel was "ahead of his time" because the statistical laws which Mendel found to govern

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The Hershey-Chase Experiment

Bacteriophages, or *phages* as they are called in the trade, are subcellular parasites of bacteria that were discovered in 1917.[10] They occupy only about one-thousandth of the volume of their bacterial host cell and hence are so small that they cannot be seen in ordinary microscopes using visible light. Phages were first seen in 1940, in the wake of the development of the electron microscope, and they were found to be tadpole-shaped particles having a head and a tail. (It was eventually established that the tail is composed of protein and that the head represents a stuffed bag whose casing is protein and whose stuffing is DNA.)

In 1938 the physicist Max Delbrück, then a postdoctoral fellow at the California Institute of Technology, started experimenting with phages, in the expectation that study of their self-replication might throw light on what Muller's essay described as "that unique property which makes a gene a gene—its ability to cause the synthesis of another structure like itself, in which even the mutations of the original gene are copied." Delbrück's work began with designing the *one-step-growth experiment,* in which he used as his experimental material a phage active on *Escherichia coli.*[11] The one-step-growth experiment showed that each phage particle infecting an *E. coli* bacterium gives rise to some hundred phage progeny particles after a brief half-hour latent period. Thus this ex-

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heredity could not be connected with the then known aspects of anatomy and physiology; and I am trying to show in the following just why it was difficult in 1944 to connect DNA with the gene. Lest it be concluded that the judgment of prematurity can be rendered only with hindsight, I shall provide an example of a recent discovery that falls within the purview of this essay and can be judged as being ahead of the *present* time. Three or four years ago there appeared reports purporting to have shown that the memory of a task learned by a trained donor animal can be transferred to a naïve recipient animal through the vehicle of nucleic acid molecules. Now whereas it is quite generally appreciated that the possibility of such memory transfer would, in fact, constitute a fact of capital importance for our understanding of the higher nervous system, these reports have so far remained without heuristic effect on brain research. For there is no chain of reasonable inferences by means of which our present, albeit very imperfect, view of the functional organization of the brain can be reconciled with the possibility of its acquisition, storage, and retrieval of experiential information through nucleic acid molecules. Thus for the community of neurobiologists there is no point in paying serious attention to these claims, or even to spend any time on checking whether they are true or false. This attitude may, at first glance, appear to be "unscientific," but it is, in fact, the very way in which science has to operate. Or, as Arthur Eddington advised his fellow scientists, it is not a good policy to put overmuch confidence in facts until they have been proven by theory. This appears to be also the view taken by Michael Polanyi (*Science,* 141 [1963], 1010) who showed that a premature discovery of his own was understandably ignored for forty years because during all that time it could not be connected with the then orthodox theories of physical chemistry.

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periment brought clearly into focus the central problem of self-replication: how does the parental phage particle manage to produce its crop of a hundred progeny during that half hour? Two years later Delbrück met Salvador Luria, then a recently arrived refugee from war-torn Europe, and Alfred Hershey, of Washington University in St. Louis. This meeting brought into being the American Phage Group, whose collective memory has been preserved in a series of autobiographical essays.[12] The members of this group were united by a single common goal—the desire to reach what Muller referred to as "the real core of genetic theory," or to extend the frame of reference of genetics beyond the billiard ball gene.

Although the intellectual foundations were laid by the Phage Group for the edifice of molecular genetics during the next dozen years, the first real breakthrough came only in 1952 with an experiment by Hershey and his young assistant, Martha Chase.[13] Hershey and Chase showed by use of phage particles labeled with radiophosphorus ^{32}P in their DNA and with radiosulfur ^{35}S in their protein that at the outset of phage infection of the *E. coli* cell only the DNA of the phage actually enters the cell; the protein of the phage remains outside, devoid of any further function in the reproductive drama about to ensue within. Thus it could be concluded that the genes of the parent phage responsible for directing the synthesis of progeny phages reside in its DNA. This second demonstration that DNA is the hereditary material had an immediate and profound impact on genetic thought.

Why did Avery's announcement that DNA is the genetic material have in its day so much less effect on genetic thought than the Hershey-Chase experiment proving the same point eight years later (with much less compelling evidence, it might be noted)? First, it was only in the late 1940's that, thanks to the pioneering efforts of Luria and Delbrück,[14] bacteria and phages came to be accepted as genuine genetic organisms to which the gene concept could be legitimately applied (Lederberg's Golden Jubilee essay still has the flavor of a missionary effort to spread that gospel). So, in 1944 many people regarded bacterial transformation as some bizarre metamorphosis without relevance to hereditary processes in higher forms. Further, and more important, the tetranucleotide theory of DNA structure still held sway, so that it was very difficult to imagine how a DNA molecule made up of monotonously repeating units containing one each of the four bases *could* be the carrier of *genetic information*. Thus even those persons who were prepared to accept in the mid-1940's the conclusions that bacterial transformation is a truly hereditary process and that DNA is really the transforming principle were wont to consider the phenomenon as a case of gene *mutation*. That is to say, they imagined that DNA is a chemical capable of

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causing specific or directed mutations rather than being the gene substance itself. But the demise of the tetranucleotide theory announced in Mirsky's essay now meant that the four types of bases can follow each other in any arbitrary order in the polynucleotide chain. Since the base composition was found to be different in DNA samples obtained from different organisms, it could be supposed at the time of the Hershey-Chase experiment that any given DNA molecule harbors its genetic information in the form of a precise sequence of the four bases along its polynucleotide chain.

With the acceptance of the parental phage DNA as the genetic material and the birth of the notion that genetic information is encoded into DNA as a nucleotide base sequence, the fundamental problem of biological self-replication could now be restated in terms of two DNA functions, *autocatalytic* and *heterocatalytic*. By means of the autocatalytic function, the phage DNA replicates its own precise nucleotide base sequence a hundredfold to generate the genes with which its progeny phages are to be endowed. And by means of the heterocatalytic function, the phage DNA directs, or presides over, the synthesis of the phage-specific proteins that are to furnish the body of its progeny. The successful elucidation of these two DNA functions was to be the work of the next decade.

The Double Helix

In 1951, one year after the Golden Jubilee, Linus Pauling, of the California Institute of Technology, discovered the basic structure of protein molecules.[15] Proteins, we might recall here briefly, are long chain molecules, built up of a sequence of twenty different kinds of *amino acids*. These amino acids are joined to each other through a chemical linkage called the *peptide* bond. The length of different kinds of protein chains present in living cells varies considerably, but on the average these chains contain about three hundred amino acids linked end-to-end. Pauling found that the three-dimensional conformation of the amino acid chain is a helix, to which he gave the name *α helix*. The discovery of the α helix represented the first great achievement in the attempt to work out protein structure by the methods of X-ray crystallography.

Pauling's success inspired James D. Watson, a twenty-two-year-old member of the Phage Group and a pupil of Luria's, to abandon the genetic and physiological experiments on phage reproduction that he had been carrying out. Watson instead decided to try to work out the three-dimensional structure of the DNA molecule, which, a few months after his decision, the Hershey-Chase experiment showed to be the carrier of the phage genes. To gain the necessary skills in X-ray crystallography,

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Watson joined John C. Kendrew of the University of Cambridge, who, like Pauling, was studying protein structure. In Cambridge, Watson met Francis Crick, a Ph.D. student, to whom it had also occurred that the three-dimensional structure of DNA would be likely to provide important insights into the nature of the gene. Watson and Crick then began a collaboration whose story is now so well known through Watson's famous autobiographical account[16] that there is little need to retell it here. Suffice it to say that in the spring of 1953 Watson and Crick discovered that the DNA molecule is a *double helix*, composed of two intertwined polynucleotide chains (Figure 2). The DNA double helix is self-complementary, in that to each adenine nucleotide on one chain there corresponds a thymine nucleotide on the other chain, and that to each guanine nucleotide on one chain there corresponds a cytosine nucleotide on the other chain. The specificity of this complementary relation devolves from hydrogen bonds formed between two opposite nucleotides, adenine-thymine and guanine-cytosine, at each step of the double helical molecule (Figure 3). The complementary base-pairing relation of the two chains thus accounted for the base composition equivalence rule noticed by Chargaff three years earlier.

On first sight, Watson's and Crick's discovery of the double-helical structure of DNA resembled Pauling's then two-year-old discovery of the α helix, particularly since the formation of specific hydrogen bonds also plays an important role in Pauling's structure. But, on second sight, the promulgation of the DNA double helix emerges as an event of a qualitatively different heuristic character. First, in working out the structure of the double helix Watson and Crick had for the first time introduced genetic reasoning into structural determination, by demanding that the evidently highly regular three-dimensional structure of DNA must be able to accommodate the informational aspect of an arbitrary nucleotide base sequence along the two polynucleotide strands. Second, the discovery of the DNA double helix opened up enormous vistas to the imagination. It was to provide the high road to "the real core of genetic theory" which, according to Muller, was still lying in the deep unknown at the time of the Golden Jubilee. Molecular genetics had now become a going concern.

Watson and Crick had concluded their letter to *Nature*,[17] in which they first described the DNA double helix, with a sentence that can surely lay claim to being one of the most coy statements in the literature of science: "It has not escaped our notice that the specific [nucleotide base] pairing we have postulated immediately suggests a possible copying mechanism of the genetic material." In a second letter to *Nature*[18] they soon told what it was that had not escaped their notice: the DNA molecule can achieve its autocatalytic function upon separation

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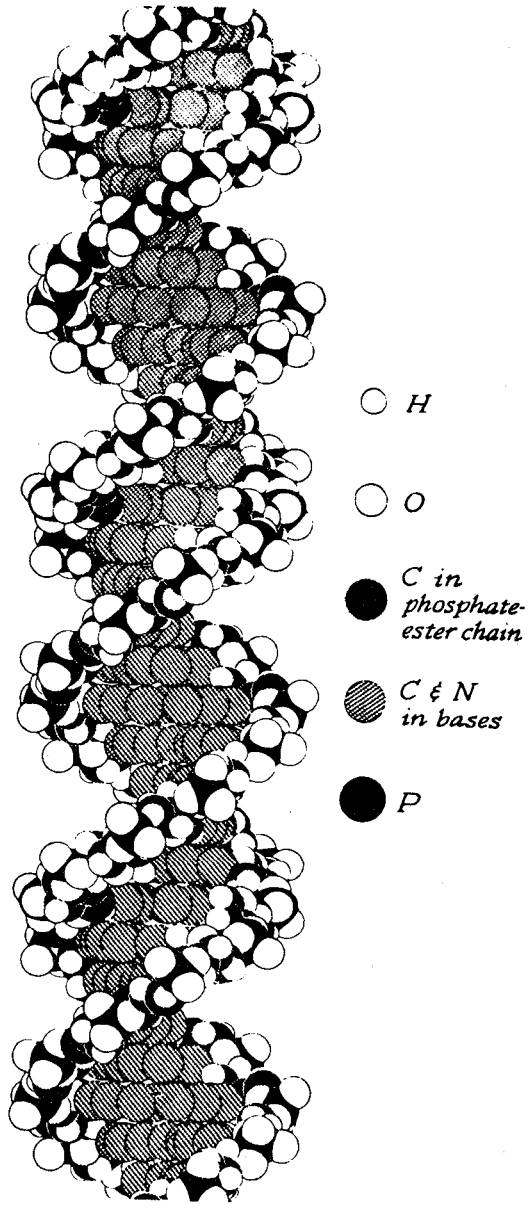

Figure 2. A space-filling model of the Watson-Crick double helix structure of DNA. (From G. S. Stent, *Molecular Biology of Bacterial Viruses* [San Francisco: W. H. Freeman, 1963].)

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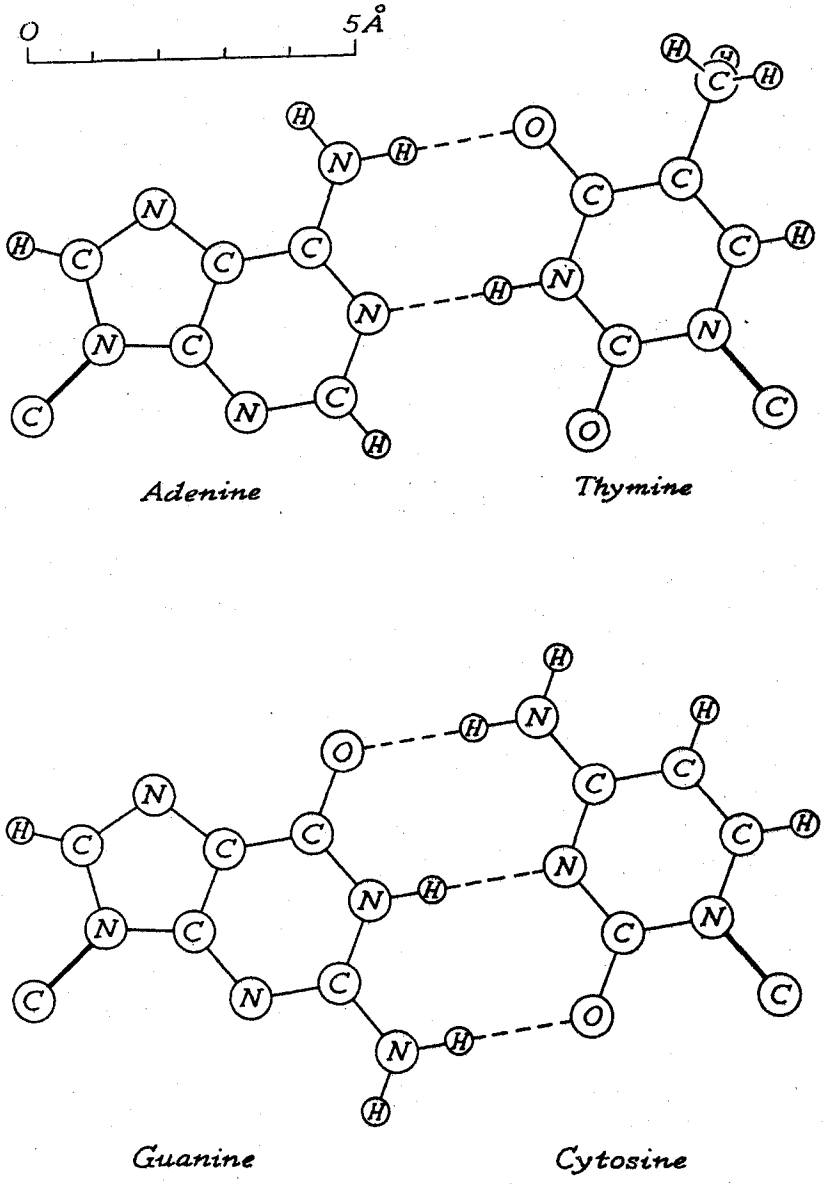

Figure 3. The complementary base pairing of adenine and thymine, and of guanine and cytosine in the double-helical DNA molecule. Hydrogen bonds are shown as dashed lines. The carbon atom of the deoxyribose to which each base is attached is also shown. (From G. S. Stent, *Molecular Biology of Bacterial Viruses* [San Francisco: W. H. Freeman, 1963].)

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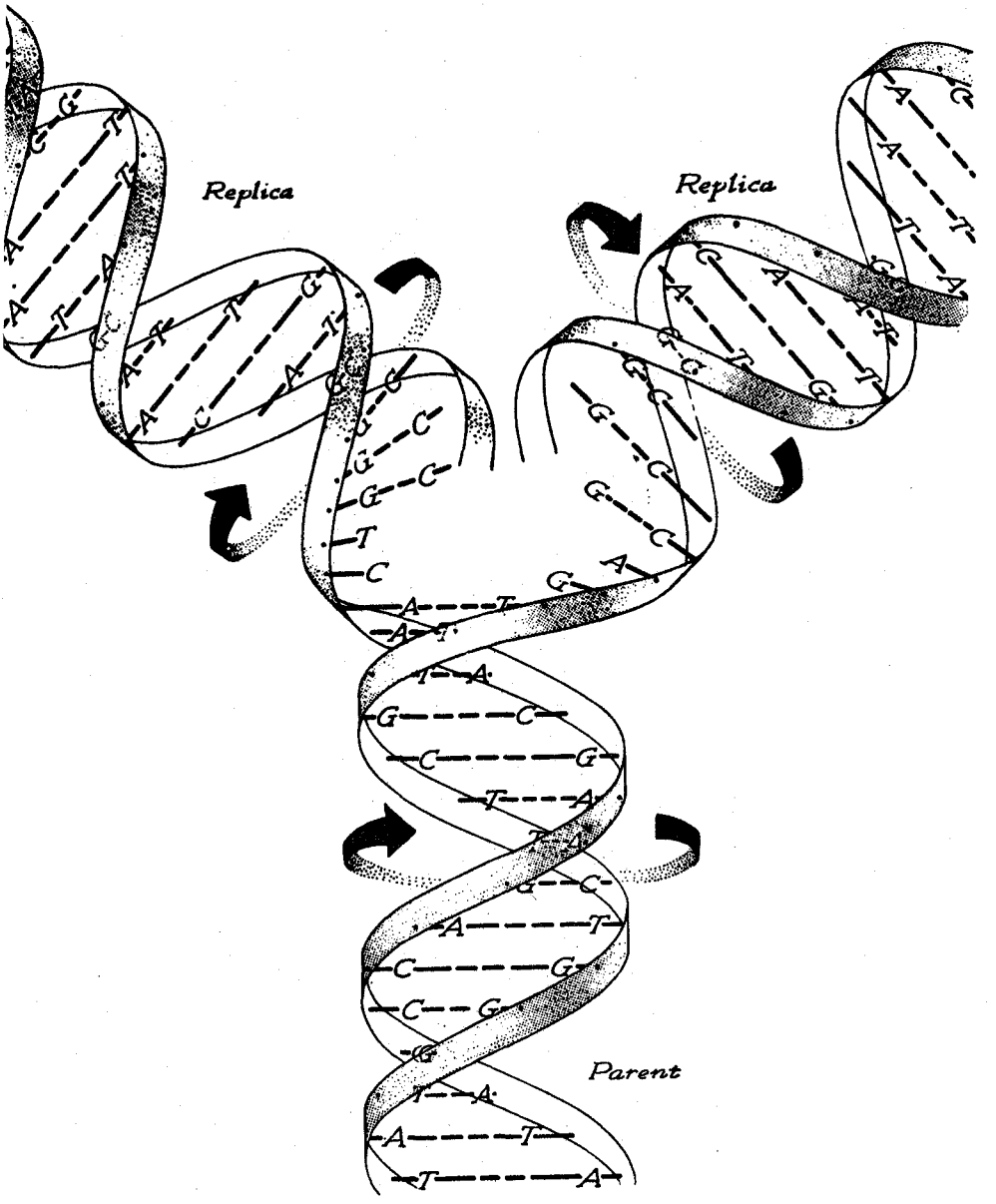

Figure 4. Replication of the DNA double helix, according to the mechanism of Watson and Crick. (From G. S. Stent, *Molecular Biology of Bacterial Viruses* [San Francisco: W. H. Freeman, 1963].)

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of the two helically intertwined, complementary polynucleotide strands (Figure 4). Each of the two parent strands would then serve as a *template* for the ordered assembly of its own complementary daughter strand, by having every nucleotide on the parent strand attract and line up for polynucleotide synthesis a free nucleotide carrying the complementary purine or pyrimidine base. Thus in the case of phage reproduction, the DNA of the infecting phage particle would undergo successive rounds of unwinding and complement addition. In this way an intrabacterial pool of replica phage DNA molecules would be built up, identical in nucleotide base sequence to the DNA of the parent phage; this pool would provide the genes for the offspring phages.

It took about five more years to prove that the solution to the problem of the autocatalytic function proposed by Watson and Crick is essentially correct. The main proof depended on the prediction[19] that under that proposal the atoms of the parental double helix ought to become distributed in a *semi-conservative* manner. That is to say, each of the two daughter molecules generated by fission of the parental DNA double helix should contain one polynucleotide chain of parental provenance and one chain synthesized *de novo*. In 1958, Matthew Meselson and Frank W. Stahl, one a graduate student of Pauling's and the other a postdoctoral student in Delbrück's laboratory at Caltech, managed to demonstrate that the DNA of *E. coli* is replicated semi-conservatively.[20] For the purpose of this demonstration, Meselson had adapted the method of *equilibrium density gradient sedimentation* to the detection of minute density differences of large molecules, particularly for measuring the density increase produced by the replacement of a common atomic species, such as nitrogen ^{14}N, by its rare heavier isotope, such as ^{15}N, in the molecules. This method was to become one of the most widely used techniques in later molecular biological research.

Reform of the Gene

Understanding of the heterocatalytic function of DNA, which from the very outset of its formulation appeared as a more complex problem than the autocatalytic function, required a rather greater effort and a somewhat longer time. First of all, it proved necessary to reform the concept of the classical gene. Today it seems impossible to make an accurate reconstruction of the historical course taken by this reform and to ascertain to whom each facet can be attributed. Many of the essential ideas were first proposed in informal discussions on both sides of the Atlantic and were then quickly broadcast to the cognoscenti by private international bush-telegraph. Months and often years elapsed before a new idea was committed to print, and then very often it was not by the

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person who had first thought of it. Nevertheless, it can hardly be doubted that here too Watson and Crick played a dominant role, as well as Seymour Benzer, then at Purdue University.

One of the points of departure of this reform was Beadle's and Tatum's one-gene–one-enzyme theory. By the early 1950's the analytical work of Frederick Sanger,[21] of the University of Cambridge, had lent direct support to the credence that a given species of enzyme represents a homogeneous class of protein molecules in which a definite number of the twenty different kinds of amino acids are assembled in a unique sequence. From this credence developed the so-called *sequence hypothesis,* which states that the exact spatial conformation of a protein molecule, and hence the specificity of its biologic function, is *wholly determined* by that unique amino acid sequence from which it is built. At the time that this hypothesis was advanced there was no proof whatsoever of its validity. But it came to be embraced immediately as molecular-genetic dogma, because it allowed rephrasing of the one-gene-one-enzyme theory in more concrete terms: the gene directs the synthesis of one enzyme by directing the assembly of the twenty kinds of amino acids into a protein molecule of given amino acid sequence. But since it was already taken for granted that the genetic information is held in the form of a particular nucleotide base sequence in DNA, it now became clear that the *meaning* of the particular nucleotide base sequence making up a sector of DNA corresponding to a gene could be nothing other than the specification of an amino acid sequence of the corresponding protein molecule. By means of incisive *fine structure* genetic experiments on a single gene of one of the *E. coli* phages, Benzer was able to gather convincing support for the notion that the gene is, in fact, a linear array of DNA nucleotides which determines a linear array of protein amino acids. It was Benzer more than anyone else who showed that the fundamental genetic unit is the DNA nucleotide base.[22]

Genetic Code

This reform of the gene concept led directly to the belief that there must exist a *genetic code* that relates the nucleotide base sequence in the DNA polynucleotide chain to the amino acid sequence of the corresponding enzyme protein. An obvious consideration quickly revealed that this code could be no simpler than one involving the specification of each amino acid in the protein molecule by at least *three* successive nucleotide bases in the DNA. Certainly there cannot exist a *one-to-one* correspondence between nucleotide bases in the DNA and amino acids in the protein molecule, because the four kinds of nucleotide bases taken *one* at a time could specify only one out of four, not one out of

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twenty, kinds of amino acids. Nor would it suffice that *two* adjacent nucleotide bases specify one amino acid, since in that case only $4 \times 4 = 16$ kinds of amino acids could be coded for by the four kinds of nucleotide bases. But four kinds of nucleotide bases taken *three* at a time provide $4 \times 4 \times 4 = 64$ different code words, or *codons*, and hence each of the twenty kinds of protein amino acids could be represented by at least one such codon in the genetic code, with the extra number of codons allowing for the possibility that each kind of amino acid is represented by more than a single codon.

These a priori insights had certainly been reached within a few months of the discovery of the DNA double helix and were first published in 1954 by the physicist-cosmologist George Gamow.[23] But it was not until 1961 that it was finally proven that the genetic code does involve a language in which a triplet of successive nucleotide bases in the DNA polynucleotide chain stands for one protein amino acid. That proof came from purely formal genetic experiments carried out by Crick[24] and his collaborators on the same phage gene which had figured in Benzer's earlier reform of the gene concept. As we shall see presently, the code was unexpectedly broken that same year.

Central Dogma

It was one thing to have formulated the general principles according to which genetic information is stored and replicated in the DNA. But it was quite another to work out the molecular mechanisms of the heterocatalytic function through which that information is realized as protein molecules. Here Watson and Crick also played a dominant role by formulating in the years 1953-1955 what came to be known as the *central dogma*. According to that dogma the heterocatalytic function is a *two-stage process*, in which the other type of nucleic acid, RNA, also becomes involved. In the first stage, the DNA molecule serves as the template for the synthesis of an RNA polynucleotide chain onto which the sequence of nucleotide bases in the DNA chain is *transcribed*. In the second stage, the RNA chain is then *translated* by the cellular machinery for protein synthesis into protein molecules of amino acid sequence specified via the genetic code. It is to be noted that an essential feature of the central dogma is the one-way flow of information,

$$\text{DNA} \rightarrow \text{RNA} \rightarrow \text{protein},$$

a flow which is never reversed.

In order to study the processes envisaged by the central dogma it became necessary to employ the methods of biochemistry to open the black box containing the cellular machinery which actually effects the

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transcription-translation drama of the central dogma. One of the first insights then provided by the application of biochemical methods was the identification of the *ribosome* as the *site* of cellular protein synthesis.[25] The ribosome is a small particle present in vast numbers in all living cells (one *E. coli* bacterium contains about 15,000 ribosomes); its mass is composed of about one-third protein and two-thirds RNA. But how is the information for specific amino acid sequences encoded in the gene made available to the ribosome in its protein assembly process? In answer to this question it was proposed in 1961 by François Jacob and Jacques Monod,[26] of the Pasteur Institute, that the RNA onto which, according to the central dogma, the genetic nucleotide base sequence is first transcribed is a molecule of *messenger RNA* (Figure 5). This messenger RNA molecule is picked up by a ribosome, on whose surface then proceeds the translation of RNA nucleotide sequence into protein amino acid sequence, codon by codon. In this translation process, the messenger RNA chain runs through the ribosome as a tape runs through a tape recorder head. While the tail of a messenger RNA molecule is still running through one ribosome, its head may already have been picked up by another ribosome, so that a single molecule of messenger RNA can actually service several ribosomes at the same time.

How the amino acids are actually assembled into the correct predetermined sequence by the messenger RNA as it runs through the ribosome had been envisaged by Crick[27] before the concept of the messenger RNA had even been clearly formulated. Crick thought it unlikely (working from first principles, as was his wont) that the twenty different amino acids could interact in any specific way directly with the nucleotide base triplet on the RNA template chain. He therefore proposed the idea of a nucleotide *adaptor*, with which each amino acid is outfitted prior to its incorporation into the polypeptide chain. This adaptor was thought to contain a nucleotide base triplet, or *anticodon*, complementary (in the Watson-Crick nucleotide base pairing sense) to the nucleotide triplet codon that codes for the particular amino acid to which the adaptor is attached. The anticodon nucleotides of the adaptor would then form specific hydrogen bonds with their complementary codon nucleotides on the messenger RNA and thus bring the amino acids bearing the adaptor into the proper, predetermined alignment on the ribosome surface.

No sooner had the adaptor hypothesis been formulated than students of the biochemistry of protein synthesis began to encounter an ensemble of specific reactions and enzymes that gradually resembled more and more the a priori postulates of that hypothesis.[28] First, a special type of small RNA molecule, the *transfer RNA*, was discovered, which contains about 80 nucleotides in its polynucleotide chain. Each cell con-

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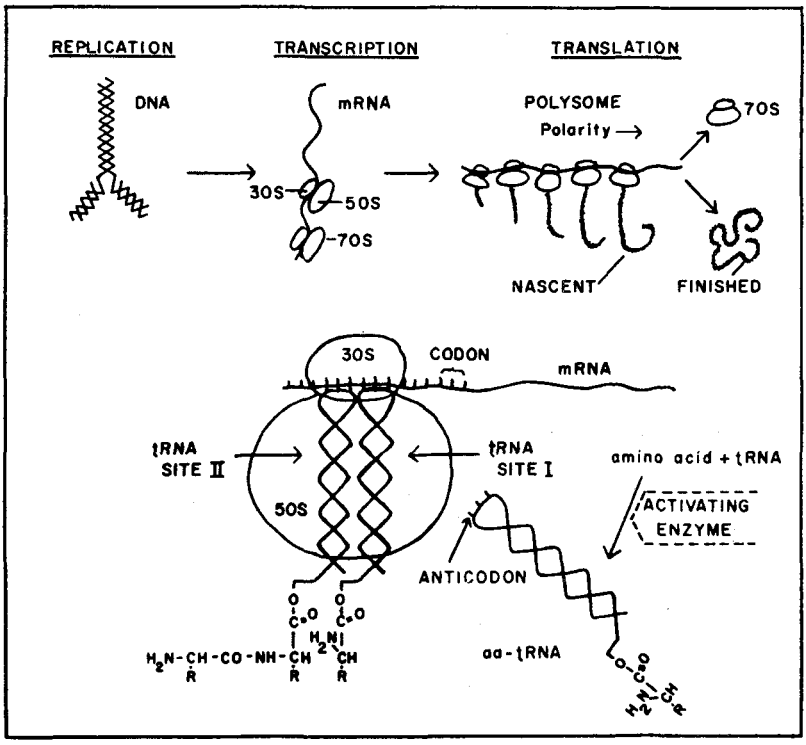

Figure 5. A summary diagram of the autocatalytic (replication) and heterocatalytic (transcription and translation) functions of DNA. *Upper left:* The DNA double helix replicates according to the semiconservative Watson-Crick mechanism. *Upper center:* The DNA nucleotide sequence has been transcribed onto a molecule of messenger RNA, or mRNA. The mRNA molecule is engaged by ribosomes, which are composed of two subunits, a smaller one called 30S and a larger one called 50S; the two subunits together constitute the intact, or 70S, ribosome. *Upper right:* Each of several ribosomes working in tandem on the same mRNA (a "polysome") translates the mRNA nucleotide sequence into the corresponding polypeptide chain. When a ribosome has translated the entire nucleotide sequence corresponding to a gene, the completed protein chain is released and the ribosome is free to attach itself to another mRNA molecule. *Lower part:* Details of the process of amino acid assembly. The nascent protein chain (here consisting merely of two amino acids) is attached to that molecule of transfer RNA, or tRNA, which figured as adaptor of the last amino acid to be added into the chain. This molecule of tRNA is in turn held to site II of the 50S ribosomal subunit. The next amino acid to be incorporated into the nascent protein chain is specified by that nucleotide triplet codon of the mRNA which faces site I of the 50S subunit. Into site I can fit only a molecule of tRNA whose anticodon matches the codon displayed by the mRNA and to which the appropriate amino acid has become attached, thanks to the recognition effected by the activating enzyme. Once the tRNA has entered site I, its amino acid is brought into juxtaposition with the last amino acid of the nascent protein chain, and the next peptide bond can be formed. When the chain has thus been elongated by one amino acid residue, mRNA and tRNA molecules move over the ribosome from right to left, to translocate the tRNA now carrying the nascent chain from site I to site II and to display the next codon at site I. (From H. K. Das, A. Goldstein, and L. C. Kanner, *Molecular Pharmacology*, 2 [1966], 158.)

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tains several dozen distinct species of transfer RNA, each species being capable of combining with one and only one kind of amino acid. This transfer RNA turned out to be Crick's postulated adaptor, since that transfer RNA species which accepts any given amino acid contains the anticodon nucleotide triplet in its polynucleotide chain which is complementary to the codon representing that same amino acid in the genetic code.

Second, a set of enzymes, the *amino acid activating enzymes,* was discovered, each of whose members is capable of catalyzing the combination of one kind of amino acid with its cognate transfer RNA molecule. Thus the set of activating enzymes which matches each amino acid with its proper transfer RNA adaptor (by means of which the amino acid is recognized in protein synthesis) evidently represents the *dictionary of heredity,* the cellular agency that "knows" the genetic code.

Breaking the Code

The actual deciphering of the genetic code began with a discovery made by the young biochemist Marshall Nirenberg at the National Institutes of Health. In the spring of 1961 Nirenberg had managed to develop a "cell-free" system capable of linking amino acids into protein molecules. This system contained ribosomes, transfer RNA, and amino acid activating enzymes extracted from *E. coli.* Though Nirenberg was by no means the first to reassemble *in vitro* the cellular machinery for protein formation, his system had one very important advantage over its predecessors: here protein synthesis depended on the addition of messenger RNA to the reaction mixture. Thus it became feasible to direct the *in vitro* formation of specific proteins by introducing into this system specific types of messenger RNA. Now when Nirenberg introduced a synthetically produced *monotonous* RNA containing *only* the uracil nucleotide (instead of the four types of nucleotide bases present in natural messenger RNA), he obtained a dramatic result. Addition of the artificial, monotonous messenger RNA resulted in the *in vitro* formation of an equally monotonous "protein," namely a "protein" containing only one kind of amino acid—phenylalanine.[29] This result could have only one meaning: in the genetic code the uracil-uracil-uracil (or in the DNA, the equivalent thymine-thymine-thymine) nucleotide triplet represents the amino acid phenylalanine.

Nirenberg announced his identification of the first codon in August 1961, at the International Congress of Biochemistry in Moscow, where it caused a sensation. (Crick later wrote that he was "electrified.") Thus at one stroke the breaking of the genetic code had become accessible to direct chemical experimentation, because now the effect of

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introducing various synthetically produced types of messenger RNA of known composition into the cell-free protein-synthesizing system could be examined. The Moscow announcement set off a code-breaking race, sometimes called the Code-War of the U3 Incident, which culminated in deciphering the definite, or at least probable, meaning of many of the 64 codons by 1963.

In 1964 Nirenberg made a second experimental breakthrough in the deciphering of the genetic code by means of his cell-free system for protein synthesis.[30] At that time he discovered that it is possible to detect in his reaction mixture the specific *binding* to ribosomes of molecules of transfer RNA carrying their cognate amino acids. In particular, he found that addition to his reaction mixture of a very short polynucleotide chain consisting of only three nucleotides, instead of messenger RNA, promoted the *specific binding* to ribosomes of those and only those transfer RNA molecules that carry the anticodon complementary in the Watson-Crick base-pairing sense to the nucleotide triplet added to the reaction mixture. Protein formation does not, of course, occur under these conditions, since the short nucleotide triplet cannot serve as the template for directing the assembly of many amino acids. By means of this new technique Nirenberg found, in confirmation of his earlier identification of the codon representing the amino acid phenylalanine, that addition of the uracil-uracil-uracil nucleotide triplet promotes the binding of phenylalanine transfer RNA to ribosomes. And since it was relatively easy to prepare by chemical methods all 64 possible nucleotide triplets, the entire code could be worked out by means of this binding method within little more than a year.

The results of Nirenberg's work, which were presently supported or confirmed by other methods, are now generally presented in the form of a table, the arrangement of which was conceived by Crick (Figure 6). It has been suggested that this table of the code represents for biology what the periodic table of the elements represents for chemistry. The important features of the genetic code are these:

(1) The code contains synonyms, in that many amino acids are coded for by more than one kind of codon. For instance, the nucleotide base triplets uracil-uracil-uracil *and* uracil-uracil-cytosine are synonyms, in that they both code for phenylalanine.

(2) The code has a definite structure, in that synonymous codons representing the same amino acid are nearly always in the same "box" (see Figure 6) of the table. That is, the synonymous codons generally differ from each other only in the third of their three nucleotides. An explanation for this aspect of the code was provided by Crick in terms of the geometry of the hydrogen bonds involved in the recognition of the messenger RNA codon by the transfer RNA anticodon at the ribosomal site of protein formation.

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1st ↓ 2nd →	U	C	A	G	↓3rd
U	PHE	SER	TYR	CYS	U
	PHE	SER	TYR	CYS	C
	LEU	SER			A
	LEU	SER		TRP	G
C	LEU	PRO	HIS	ARG	U
	LEU	PRO	HIS	ARG	C
	LEU	PRO	GLUN	ARG	A
	LEU	PRO	GLUN	ARG	G
A	ILEU	THR	ASPN	SER	U
	ILEU	THR	ASPN	SER	C
	ILEU	THR	LYS	ARG	A
	MET	THR	LYS	ARG	G
G	VAL	ALA	ASP	GLY	U
	VAL	ALA	ASP	GLY	C
	VAL	ALA	GLU	GLY	A
	VAL	ALA	GLU	GLY	G

Figure 6. The table of the genetic code. In this table the letters U, C, A, and G represent the four kinds of nucleotides, containing the bases uracil, cytosine, adenine, and guanine. The three- and four-letter abbreviations represent the twenty kinds of protein amino acids. The codon corresponding to any given position on this table can be read off according to the following rules: The base of the first nucleotide of the codon is given by the capital letter on the left, which defines a horizontal row containing four lines. The base of the second nucleotide is given by the capital letter on the top, which defines a vertical column containing sixteen codons. The intersection of rows and columns defines a "box" of four codons, all of which carry the same bases in their first and second nucleotides. The base of the third nucleotide is given by the capital letter on the right, which defines one line within any given horizontal row. The triplets UAA, UAG, and UGA are "nonsense" codons, to which there corresponds no amino acid.

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(3) The code is very nearly universal. Though most of the code was deciphered by use of the protein-synthesizing machinery of *E. coli*, later tests showed that the results are very much the same whether the transfer RNA and amino acid activating enzymes (the agency that "knows" the code) are obtained from bacterial, plant, or animal sources (including mammals). The universality of the code among contemporary living forms shows that the code has remained unchanged over a very long period of organic evolution. One explanation which has been offered for the, at first sight surprising, evolutionary permanence of the code is that any genetic mutation engendering a change in the code—by means of which evolution of the code would have had to proceed—would necessarily be lethal to the organism in which it took place: such a mutational alteration of the code would cause a sudden change in the amino acid sequence of *all* of the protein molecules of the mutant organism. And such a wholesale change would be most unlikely to be compatible with survival of the organism. Another possible explanation for the evolutionary permanence of the code is that there exists some as yet unfathomed geometrical or stereochemical relation between the anticodon nucleotide triplet and the amino acid which it represents. Indeed, if such a relation exists, it would be bound to hold one of the keys to understanding the origin of life.

The Operon

The developments recounted so far have revealed the *qualitative* aspect of the heterocatalytic function: how the nucleotide base sequence carried by the DNA is finally translated into the predetermined amino acid sequence. However, there must pertain to the heterocatalytic function also a *quantitative* aspect: how the DNA manages to govern the synthesis of appropriate amounts of the different proteins whose structure is encoded into it. For it is an easily ascertainable fact that in *E. coli* the number of protein molecules read off one gene per generation can exceed by more than ten-thousandfold the corresponding number read off another gene in the same cell. Furthermore, the rate of translation of any given gene is not always the same, being very high under some physiological conditions and very low under others.

The framework for understanding this quantitative aspect, which was not really covered by the original formulation of the central dogma, was finally provided in 1961 by the *operon theory* of Jacob and Monod[26] (Figure 7). In order to explain the functional regulation of the genes, this theory envisages that a group of genes, or an *operon*, is subject to coordinate control. The genes belonging to the same operon occupy contiguous sectors of the DNA; that is, they are closely linked

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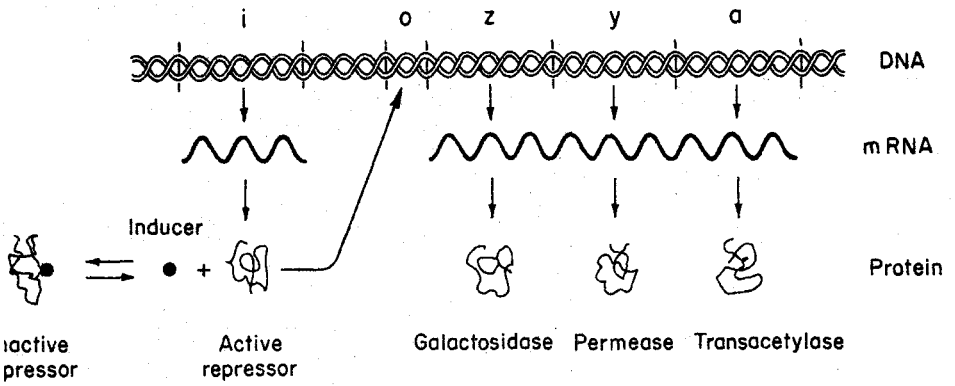

Figure 7. The operon theory of Jacob and Monod, as applied to the lactose fermentation genes of *E. coli*. Three contiguous genes, *z*, *y*, and *a*, code for the protein structure of three enzymes, galactosidase, permease, and transacetylase, and share a common operator gene *o*. A gene *i* codes for the protein structure of the repressor, which can attach to and thus "close" gene *o*. Combination with an inducer, such as lactose, inactivates the repressor, thus preventing it from "closing" *o*. (From G. S. Stent, in *The Neurosciences* [New York: Rockefeller University Press, 1967].)

and share a common, special regulatory segment of DNA, their *operator*. This operator, which is located at one extreme of the operon group of genes, can exist in two states: open or closed. As long as the operator is open, messenger RNA can be transcribed from all the genes of the operon, and hence synthesis of the protein species encoded into these genes may proceed on the ribosomes into which these messenger RNA molecules are fed. As soon as the operator is closed, transcription of the messenger RNA, and hence synthesis of the corresponding protein species, ceases. Thus the rate of translation of the genes belonging to any operon depends on the fraction of the total time during which their operator happens to be open. Now whether the operator is open or closed in turn depends on whether it has interacted with a *repressor* protein, itself the product of a special regulatory gene. Combination of the operator segment with its related repressor inhibits messenger RNA transcription of the genes of that operon and therefore closes the operator. Hence, in the last analysis, the rate of translation of any gene is a function of the intracellular concentration of active repressor capable of combining with the operator segment of DNA to which that gene is linked.

Thus, by the mid-1960's the general nature of both autocatalytic and heterocatalytic functions of the DNA was understood.[31] Through for-

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mation of complementary hydrogen bonds DNA achieves both functions by serving as a template for the synthesis of replica polynucleotide chains, making DNA chains for the autocatalytic and RNA chains for the heterocatalytic function. RNA, in turn, completes the heterocatalytic function by formation of complementary hydrogen bonds with the anticodons of transfer RNA molecules in the amino acid assembly processes. The real core of genetic theory was lifted from the deep unknown in which Muller had found it to lie only fifteen years earlier. We now *do* have actual knowledge of the mechanism underlying that unique property which makes a gene a gene: Formation of complementary hydrogen bonds seems to be all there is to how like begets like.

Alas, the very success of molecular genetics in explaining one of the most profound mysteries of life in terms of workaday chemical reactions altered the spiritual qualities of this field. Molecular genetics now presents an integral canon of biological knowledge which must be preserved and passed on to succeeding generations in the academies. Moreover, as a subject for scholarly research it is far from exhausted, and its practical exploitation in such biotechnological domains as agriculture and eugenics has, as yet, barely begun. But its appeal as an arena for heroic strife against the Great Unknown is gone. And in contrast to classical genetics, which at the apogee of its development still harbored the enigma of the gene as a skeleton in the closet, molecular genetics seems to have left no transcendant legacy. So the would-be explorer of uncharted territory must direct his attention elsewhere. One of the most formidable unsolved problems of biology recommending itself to such romantic types is embryology. Understanding the processes responsible for the orderly development of the fertilized egg into a complex and highly differentiated multicellular organism still seems to boggle the imagination. The recent course of embryological research, however, suggests that it may be just more of the same old molecular genetics, albeit at a much more complicated level.

There now seems to remain really only one major frontier of biological inquiry which still offers some romance of research: the higher nervous system. Its fantastic attributes continue to pose as hopelessly difficult and intractably complex a problem as did the gene a generation ago. Indeed, the higher nervous system presents the most ancient and deepest biological mystery in the history of human thought: the relation of mind to matter. Increasing numbers of veteran molecular geneticists are now turning their attention to the higher nervous system in the hope of finding relief from jejune genetic investigations along more or less clearly established lines. They have good reason to hope that, unlike the quest for fathoming the gene, the quest for understanding the brain will not soon reach a disappointingly workaday denouement. For since

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the mind-matter mystery is not likely to be amenable to scientific analysis, the most interesting attribute of life may never be explained.[32]

I am indebted to Curt Stern and Alfred E. Mirsky for helpful criticisms of the manuscript of this essay.

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28. M. B. Hoagland, P. C. Zamecnik, and M. L. Stephenson, "Intermediate Reactions in Protein Biosynthesis," *Biochimica et Biophysica Acta,* 24 (1957), 215. M. B. Hoagland, M. L. Stephenson, J. F. Scott, R. J. Hecht, and P. C. Zamecnik, "A Soluble Ribonucleic Acid Intermediate in Protein Synthesis," *Journal of Biological Chemistry,* 231 (1958), 241.

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30. M. Nirenberg and P. Leder, "RNA Codewords and Protein Synthesis: The Effect of Trinucleotides Upon Bringing sRNA to Ribosomes," *Science,* 145 (1964), 1399.

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31. By that time the Cold War had eased, both in its strategic and its genetic aspects. Molecular genetics had become definitely an in-subject in the Soviet Union, thanks in part to the final fall from favor of Lysenko after the dismissal of N. Khrushchev. But it seems likely that in any case the "materialist" concrete DNA gene of molecular genetics (and the possibility of inheritance of "acquired" characters through phenomena such as the DNA-mediated bacterial transformation) is more palatable to dialectical-materialist thought than the "idealist" abstract gene of classical "Mendelism-Morganism." See Zh. Medvedev, *The Rise and Fall of Lysenko* (New York: Columbia University Press, 1969).

32. To readers interested in the development of genetic thought, I strongly recommend A. H. Sturtevant, *A History of Genetics* (New York: Harper and Row, 1965). Some of the philosophical issues attending the rise of molecular genetics are treated in M. Delbrück, "A Physicist Looks at Biology," *Transactions of the Connecticut Academy of Sciences,* 38 (1949), 173 (reprinted, see note 12); in G. S. Stent, "That Was the Molecular Biology That Was," *Science,* 160 (1968), 390; and in D. Fleming, "Emigré Physicists and the Biological Revolution," in D. Fleming and B. Bailyn, eds., *The Intellectual Migration: Europe and America, 1930-1960* (Cambridge, Mass.: Harvard University Press, 1969), 152. Professional introductions to molecular genetics are provided by J. D. Watson, *The Molecular Biology of the Gene* (New York: Benjamin, 1965), and G. S. Stent, *Molecular Genetics* (San Francisco: W. H. Freeman, 1970). A fuller treatment of the notions touched on in the closing paragraphs of this essay can be found in G. S. Stent, *The Coming of the Golden Age: A View of the End of Progress* (New York: Natural History Press, 1969).

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ROBERT OLBY

Francis Crick, DNA, and the Central Dogma

THIS ESSAY does not present a definitive account of Francis Crick's contribution to molecular biology, but rather is an attempt at a brief description of how Crick, a British physicist caught up in the war effort, came into biology; how he collaborated with James D. Watson, the Chicago-born biologist, in discovering the structure of DNA; and how Crick went on to play a central role in the formulation of the theory underlying molecular biology today. Several of his scientific contributions have been omitted in order to focus attention on the double helix, the sequence hypothesis, the central dogma, and the genetic code.[1]

Born in June 1916, during the First World War, Francis H. C. Crick grew up in those uncertain interwar years which were marked by disillusionment, the slump, and financial ruin. Many a family business had to be closed down, including the Northampton boot and shoe factory run jointly by Francis' father, Harry, and his uncle, Walter. The shoe shops which Harry Crick continued to run failed to restore the wealth of the family, and to preserve the tradition of Mill Hill schooling for all the family, the Cricks moved close to the school so that Francis' younger brother, Tony, could become a day-boy; and Francis, who left the school in 1934, was thus able to commute daily to University College, London. Michael Hart, headmaster of Mill Hill School, recalls that Francis Crick's father was an Old Millhillian, as were three of his uncles. Of Francis he said: "We remember vividly all the characteristics (piping shrill voice and laughter) described by his contemporaries at Cambridge today. He took Higher Certificate and gained distinctions in Physics and Maths for Science, and also a pass in Chemistry. He was a highly competent VI former who was expected to do very well, but we had no real expectation of his future brilliance."[2]

The Crick family was well-to-do middle class; Francis' father and his uncles were fond of bridge and tennis parties and were good Congregationalists. But with the exception of Francis' grandfather,

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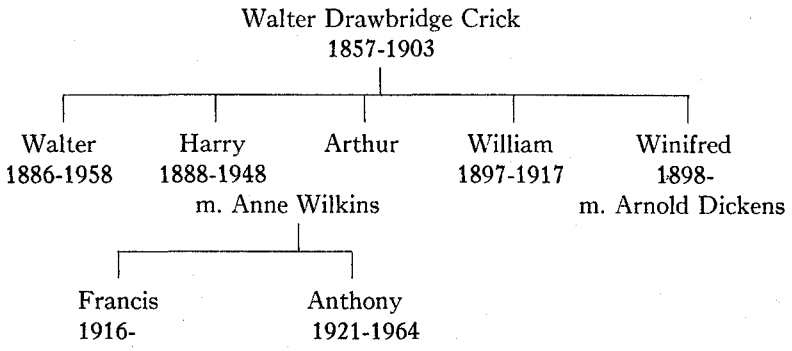
Walter Drawbridge Crick, a prominent member of the Northamptonshire Natural History Society and Fellow of the Geological Society, Francis was alone in the intensity of his scientific curiosity. It was W. D. Crick who reestablished the family name in Northamptonshire when he rose from the position of clerk in the Goods Department of the London and North-Western Railways Co. to director of the boot-and-shoe firm of Latimer Crick and Co.[3] and owner of the fine house, Nine Springs Villa, with its spacious gardens, in a select part of Northampton. All this he achieved in a short life of forty-six years, during which he also found the time to collect liassic fossils, freshwater bivalves, Cruikshankiana, books, porcelain, and furniture. As a student of freshwater bivalves he had the distinction of a mention by Charles Darwin.[4]

Of his father's generation (see Table 1) Francis said that Willie (killed in the 1914-1918 war) was reckoned the cleverest, but all were bright. Uncle Walter, who had taken over the direction of the factory at the age of nineteen was deeply disturbed by the events of the slump, blaming the bankers whose high interest rates had reduced liquidity and deflated the economy with ruinous efficiency. Although he became quite fanatical on this subject, the pamphlet *Abolish Private Money, or Drown in Debt*, written with Frederick Soddy, does demonstrate considerable command of the data and the ability for forthright expression.[5] Neither Walter nor Harry was interested in making money; both would have preferred a different career. While Harry remained in England to supervise the London shoe shops, Walter emigrated to the United States, where he spent the last eighteen years of his life as a sales agent for shoe manufacturers.[6] Only Uncle Arthur had a flair for business, and he amassed a fortune as the owner of a chain of drugstores in Kent.

Table 1.



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What restricted the intellectual interests of this generation of Cricks was the fact that they never went to a university, a result, in part, of the early death of their father. Their interests, too, were no doubt determined by the nonacademic, nonconformist circle in which they moved. Harry Crick had wanted to become a doctor, but his real passion was tennis;[7] such was his zeal that he would play secretly on Sundays. As church secretary he could not have done this openly, and Francis, as the son of so respected a figure, did not find it easy to evade regular attendance at services. His resistance to this duty led him at an early date to take the antivitalist's position, a stand which was later to make the field of molecular biology particularly attractive (and, as we shall see, it was he who gave a rigorous statement of the reducibility of cell physiology to chemistry and physics in the sequence hypothesis and the central dogma).[8]

It would be a mistake to describe Crick's family as boring and humdrum. While Francis found something lacking in his family circle at Northampton, the family found something lacking in him. This was not due to conflict between radical and bourgeois ideas, for the family admired Northampton's free thinker, Charles Bradlaugh, and Uncle Walter took sides with the workers over the causes of the slump. But there was no one in the family circle with whom Francis could talk about the scientific topics which fascinated him. While some people operate best in seclusion, others need sounding boards, critics, catalysts; for them ideas are formed to be discussed, and part of their zest for life comes from the joy of communicating ideas. Crick undoubtedly belongs to the latter group. A part of what one might call his style is intense and prolonged discussion, something which his good-natured and forthright family could not provide. What interests they had were either at the natural history level or, as in the case of his college-trained mother, on the arts side. But to the young Crick science was everything; the family called him "crackers," so lopsided did his interests appear.[9]

As a schoolboy he had worked quite hard, and he took his work at University College seriously. But he cannot now recall that the physics course stimulated him in any special way: it was too early for the exciting developments in the quantum theory to permeate undergraduate physics courses. Although Professor E. N. da C. Andrade gave probably the best course on the Bohr theory of the atom in those days (he was author of the definitive book, *The Structure of the Atom*, 1923), no more than one question on this theory appeared in the physics final examinations; the remainder were on more traditional topics, such as "Describe a method suitable for measuring the viscosity of a gas," "Outline the experiments that would have to be carried out to enable the mass of the earth to be determined."[10]

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Naturally a first was expected of Crick, so there was general disappointment when he was awarded only a good second in the physics finals of 1937.[11] As a graduate student he was given the task of building an apparatus with which to measure the viscosity of water at temperatures above 100°C. Since the 1930's Professor Andrade had paid special attention to viscosity because of his interest in the liquid state, and he wanted to test his assumption of 1934 that the viscosity of a liquid "is proportional to the average number of molecules possessing the minimal kinetic energy necessary for the transmission of momentum.[12] This implied a linear relationship between log η and 1/T at constant volumes. The water had to be studied at above 100°C because at lower temperatures, as a result of hydrogen bonding, its molecules depart widely from ideal conditions.

So for two years Crick busied himself building a copper sphere containing water at high temperatures. When the sphere was oscillated, the damping effect of the water was measured, from which its viscosity was to be calculated. This work was near completion when war was declared, the laboratory was closed, and Crick joined the Admiralty Research Laboratory at Teddington. In the first year of the war a mine (at that time the Germans were using naval mines as bombs) fell on University College and destroyed his viscosity apparatus. In a report dated 1946 Professor Andrade wrote of Crick: "He showed great ability as an experimental and theoretical physicist, has an ingenious and lively mind and shaped very well as a research student. He is a very able physicist with plenty of initiative."[13] Not surprisingly he had been awarded the Carey Foster Research Prize in Physics in his second year of Ph.D. work.

At Teddington, Harry Massey (now Sir Harry) had collected a crack team of scientists to work on the design of circuit-operating systems for the firing of mines which would defeat enemy countermeasures. Here all members of the group learned to value the clear and incisive mind of Crick and his strong physical sense. When the team moved down to Havant, near Portsmouth, to join the Mine Design Department with its long-established naval staff, there were frequent tensions, and Massey found himself soothing officers hurt when Crick had told them they were talking nonsense. One incident which illustrates Crick's ability to see straight through to the heart of the matter was when Massey asked his advice about how to overcome the powerful magnets in German ships which activated the British mines at a safe distance from the ships. At once Crick replied that all you had to do was to lay some mines so insensitive to the magnets that they would only be fired right under the ships. Most of the naval staff could not see the sense of laying mines too weak to blow up a ship; they failed to see

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that the problem was a statistical one, the aim being to put out of action the maximum number of minesweepers.[14] The success of the Mine Design Department is shown by the fact that throughout the war 30 per cent of German shipping was in dry dock undergoing repairs. By the time sonic, magnetic, and electric operators were being used for British mines the German ships were trailing an army of devices to counter all such mechanisms—magnets, alternating circuits, cannons, and clappers.

When the war was over in 1946 Crick stayed on as an Admiralty scientist working in the Naval Intelligence Division in London, with the long-term aim of going into fundamental research either in particle physics or physics applied to biology. Seven years in the Admiralty made him competent in hydrodynamics and magnetism but not in fundamental physics, still less in chemistry or biology. Like so many scientists, he had found the first five years of his university career dull. On top of this was the financial effect of the depression; and had it not been for the generosity of Uncle Arthur, and his aunt, Miss Ethel Wilkins, his studies, like those of Pauling before him,[15] would have been interrupted. As it was, their generosity eased things for Crick before and during the war and in Cambridge afterwards.

Just how little Crick knew about the world of chemistry in those days is illustrated by his reaction to a speech of Linus Pauling's in 1946 on modern structural chemistry. Had Crick then known who Pauling was he would have expected to be impressed by his speech, but never having heard of Pauling before he was simply surprised and delighted at the view expressed by this structural chemist on the role which weak intermolecular forces will be shown to play in physiological processes. Pauling had said: "I believe that usually the specific physiological properties of substances are determined not by these strong intramolecular forces, but instead by the weak forces—van der Waals forces, hydrogen bonds—which operate between molecules."[16] Clearly Crick was already in 1946 receptive to the extension of orthodox physical chemistry into biology rather than to the search for unorthodox long-range forces and the like.

It was reading such articles and talking to his naval officer friends about antibiotics and similar subjects which convinced him that biology was what interested him. Also, he had read Erwin Schrödinger's book *What Is Life?* This noted physicist had asked "How can the events in space and time which take place within the spatial boundary of a living organism be accounted for by physics and chemistry?" and had concluded that "The obvious inability of present-day physics and chemistry to account for such events is no reason at all for doubting that they can be accounted for by those sciences."[17] As a physicist used to

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statistical laws in which the regularities in the behavior of particles result from the average behavior of large numbers of them, Schrödinger failed to see how so small a number of atoms as the thousand or so in the sensitive area of a gene could show such extraordinary constancy. Had they been regularly repeating crystalline polymers he could have understood it, but since the "hereditary codescript" must be "aperiodic," he concluded that "we must be prepared to find a new type of physical law prevailing in life."[18] The way out of the dilemma lay in the concept of template molecules upon which complementary aperiodic molecules can be laid down under the influence of weak intermolecular bonds. Schrödinger deliberately presented the problem from the point of view of one who doubted that bonds exist and who thus viewed the gene as a collection of unbound atoms. He went on to attribute the constancy of the gene to its crystalline-solid constitution, and there is no doubt that this concept had an impact on those physicists who read his book and turned to biology.

What impressed Crick about the book at the time was that "fundamental biological problems could be thought about in precise terms, using the concepts of physics and chemistry." One was left with the impression that "great things were just around the corner."[19] This, then, was a general impression, but the significance of Schrödinger's statement on the hereditary codescript did not strike Crick until after the DNA structure was out. Then he wrote to Schrödinger enclosing a reprint of the 1953 *Nature* paper, but received no reply. Professor Cyril Darlington also recalled meeting Schrödinger at this time and found him apathetic about the problems he had earlier discussed with such enthusiasm.[20]

A more powerful stimulus than *What Is Life?* leading Crick to biology was the "religious" one of an atheist who wanted "to try to show that areas apparently too mysterious to be explained by physics and chemistry, could in fact be so explained. That is why (having decided that since I knew so little anyway I had a free choice of subject!), I selected as the two regions: (1) the borderline between the living and the non-living (molecular biology as we would call it to-day) and (2) the brain. After a great struggle I chose the former because it seemed nearer to things I knew about already."[21]

Entering Biology

It was through Massey that Crick was introduced to the physiologist, A. V. Hill, and Sir Edward Mellanby, Secretary to the Medical Research Council, to whom he went for advice on how to get into biological research. By this time J. D. Bernal's wonderful work on the

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X-ray crystallography of proteins and nucleic acids had come to Crick's attention, and he was anxious to join Bernal's group at Birkbeck College, London. Now the way seemed clear to escape from magnetism and hydrodynamics into X-ray crystallography. But when Crick went to Birkbeck and asked the secretary if he could see Bernal with a view to working under him, he was cut down to size by the reply, "Don't you realize that people from all over the world want to come and work under the Professor?"[22] He retired abashed and sought the advice of A. V. Hill. What followed is described by Mrs. Sanderson of the Medical Research Council.

Crick explained to Hill that he was particularly attracted by the application of physical methods to biological products—as typified by Bernal's work on the structure of viruses—and with encouragement from Professor A. V. Hill he came to see Sir Edward Mellanby in the summer of 1947 to discuss his future. Sir Edward was obviously impressed by Crick's approach, but pointed out the difficulties of placing a man of his standing in the biological world at a commensurate salary; Crick was in no way discouraged by the situation and indicated that he would be prepared to enter the research field by means of an MRC studentship. All such studentships were fixed at £350 untaxed, regardless of age and experience.

Sir Edward assured him that an application sponsored by Professor Bernal would go through automatically, but suggested that in the latter's absence abroad, Crick should ask Professor Hill to speak on his behalf. He went on to surmise that at some future date Crick might well consider the possibility of joining Wyckoff's team of workers in Washington and Crick endorsed this view, expressing his own particular interest in their kind of inter-disciplinary approach to biophysics.

Crick's application for a research studentship was warmly supported by Professor A. V. Hill, but it had not been possible in Professor Bernal's absence to arrange supervision at University College. Professor Hill was in fact of the opinion that Cambridge would be a preferable centre, in view of the considerable amount of work in progress there which would give Crick the necessary experience in the biological field. In his report on Crick dated July 9, 1947 Hill stated that: ". . . by far the best place for him if someone would father him and his work there would be Cambridge. There he could form the necessary biological knowledge while beginning to learn about and apply the various techniques necessary for the purpose he has in mind." As the application was to be considered by Council within a few days, there was no time to explore the question of a supervisor at Cambridge and, quite exceptionally, the Council awarded him a research studentship without any place of work being specified. They formed a most favourable impression of his originality and potential which is clearly expressed in his application; he wrote:

> "The particular field which excites my interest is the division between the living and the non-living, as typified by, say, proteins, viruses, bacteria and the structure of chromosomes. The eventual goal, which is somewhat remote, is the description of these activities in terms of their structure, i.e., the spacial distribution of their constituent atoms, in so far as this may prove possible. This might be called the chemical physics of biology."

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After a further meeting with Sir Edward, it was arranged that Crick should visit Cambridge and discuss the whole matter of his future in the first place with Dr. (now Dame) Honor Fell at the Strangeways Research Laboratory; Sir Edward also suggested that he should meet Professor Keilin, Sir Lawrence Bragg and Professor Roughton to talk about his ideas; it was made clear that he himself would have to work out his own programme. Dame Honor's view was that Crick would be able to gain the necessary experience at Strangeways, and it was arranged that he should start work there under her supervision in September 1947; he then embarked on post-graduate studies in the biological field to complement his research interests and at the same time began part-time research on aspects of the mechanism of cell division.[23]

The Strangeways Institute had been founded by F. S. P. Strangeways in 1909 to investigate the cause of arthritis. When it became clear that apart from the classification of the various forms of arthritis, no progress was being made, the trustees switched to a broader policy with more fundamental research. It was here that the technique of tissue culture which Ross Harrison had invented in 1905 was applied to cartilage cells from arthritic joints. Here R. G. Canti took the first film of cell division, Arthur Hughes used phase-contrast cinematography to demonstrate the movement of mitochondria, C. H. Waddington carried out the first induction in mammalian embryos, and Chambers and Honor Fell used micromanipulation to study the role of the nucleus in the cell. Today the institute is doing exciting work on membrane structure, but when Crick arrived, the threadbare topic of the viscosity of protoplasm was a favored subject. A relic from the days when the physical ultrastructure of protoplasm was thought to hold the key to vital functions, studies of the viscosity of protoplasm have consistently failed to yield hard facts except in the field of plant sensitivity, where it is thought to play a part in perception. The great names of the past on the structure of protoplasm—Lewis Heilbrunn, William Seifriz, and Herbert Freundlich—have been forgotten, and along with them the "brush-heap" (Seifriz) and "framework" (Frey-Wyssling) theories of structure.

Arrival in Cambridge

When Crick arrived on the scene Dr. Fell suggested that he work with Arthur Hughes, who was studying the movement of magnetic particles ingested by chick fibroplasts in tissue culture. Crick jumped at this opportunity, since it offered clear-cut experiments of a physical nature in the uncertain green-thumb world of tissue culture. With such work he reckoned he could come to grips with the strange world of biology without the distraction of having to learn more physics. Crick had escaped from the viscosity of water only to trap himself in the

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viscosity of protoplasm: just as he spent two years on water, he now spent two years on protoplasm. To this day his friends tease him about his "twist," "drag," and "prod" games with ingested particles, experiments which led to seventy-two pages in the journal *Experimental Cell Research* on what he has always regarded as inconsequential work. Be that as it may, his analysis of previous work and theories of structure exhibits the same incisive mind so characteristic of his later work.[24]

Also, the years 1947-1949 were used by Crick to read widely, and on the basis of this self-education in biology and chemistry Crick formed many of his ideas on molecular biology. This was no case of casual browsing through the literature, but sustained, intensive, and critical study, which was carried on most weekends as well as in the evenings. At this stage he recognized the relation between gene and gene product, an enzyme, as of crucial importance, though as a physicist he regarded the structure of proteins as the most appropriate field for him.

This work at Strangeways was not presented as a thesis, for he had deliberately not embarked on a Ph.D. for fear he would be tied down too much. Now he had to decide whether to stay on to join Avrion Michison and M. M. Swann who were working on mitosis, or go to the Cavendish Laboratory to work under Max Perutz whom he had met through his friend George Kreisel, the mathematician. What happened we learn from the records of the Medical Research Council.

In the early part of 1949 Crick wrote to Sir Edward asking if he would see him again to discuss his work at Cambridge. Sir Edward agreed and during their subsequent meeting, Crick revealed that his interests were diverging from the biological aspects of biophysics to the purely physical side and that what he really wanted was to join the Council's Unit at the Cavendish Laboratory under Dr. Perutz—who was also very anxious to have him—and to carry out research using methods of X-ray analysis for determination of the structure of protein and allied molecules. Crick's studentship was nearing the end of its second year and Sir Edward told him that while the Council could not assume responsibility for looking after him when his training was over, he had better apply for a further year's extension and await events.

These were not long delayed; a few weeks later Sir Lawrence Bragg, then Head of the Cavendish Laboratory, wrote to Sir Edward as follows:

> "I have just been having a talk with F. H. C. Crick, who holds one of your Research Studentships. He has been working in the Strangeways, and now wishes to get some research experience in Perutz's team. If you wish him to do this I shall be very glad to have him in the Laboratory. I think he would be a useful member of the team, and also that he would be getting the kind of experience he appears to desire. I am leaving it to Perutz and Crick to consult you about the transfer; this is merely to say that as far as I am concerned I should welcome the arrangement."

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Sir Lawrence's letter was closely followed by an application on Crick's behalf from Dr. Perutz, who wrote:

> ". . . I have known him ever since he decided to enter the field of biophysics and know that he has always been keenly interested in the problem of protein structure, and would have liked to join our Unit from the start, but was advised to gain some experience with living materials before making a final decision about his future line of research. After a thorough study of the subject he has now decided that X-ray analysis of protein structure really is the field that attracts him most.
>
> "I should be very glad to have Crick. I had many conversations with him and he has always struck me as an exceptionally intelligent person, with a lively interest, a remarkably clear analytical mind and a capacity for quickly grasping the essence of any problem. At yesterday's meeting of the Biophysics Committee Dr. Hughes reported on the results Crick had obtained in his research at the Strangeways Laboratory. This research is now completed and Crick is writing it up for publication. He thinks that he will be ready to join us in June or July.
>
> "I am not quite sure what form of appointment the Council would consider appropriate. Having worked for the Admiralty during the war, Crick is much older than a man in his position would normally be—he is 32—and has family obligations. If he were younger, a research studentship would undoubtedly be the right thing for him, but as it is he would be much happier to have a definite appointment. On the other hand, if the Council does not consider him ripe for one, I know that he would be glad to continue with his present studentship for another year."

In the event, Crick was offered an unestablished 3 year appointment on the scientific staff of the Council's Unit and started work there in June 1949, relinquishing his research studentship at the same time. He has not in fact registered for a Ph.D. degree while working at Strangeways and the reasons are explained in a letter from Dr. Perutz requesting the Council's permission for him to do so after joining the Unit, as follows:

> "I should like to consult your advice regarding the position of Mr. F. H. C. Crick. Crick has been with me for over a year now, and I am rather anxious that he should register as a research student and take a Combridge Ph.D. since this would considerably improve his future prospects. You may remember that he held an MRC studentship while he worked at the Strangeways Laboratory before he joined me. At that time, however, he did not make use of the possibility to become a research student, first because he did not think his research problem was a suitable subject for a Ph.D. thesis, and secondly, because difficulties arose through the Strangeways Laboratory not being a University Department.
>
> "I wonder if Crick could have the Council's permission to become a registered research student while holding his present position, and if so, whether the Council would be willing to assist him with the payment of fees."

Permission was given for Crick's registration.[25]

When Crick, a bold thirty-three-year-old graduate student of Gonville and Caius College, knowing no X-ray crystallography, having no

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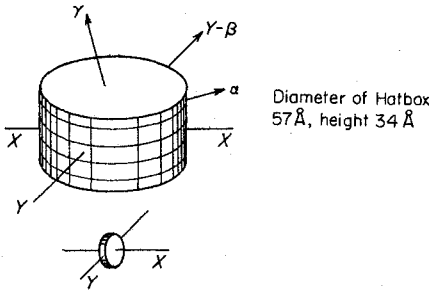

Figure 1. Diagrammatic model of haemoglobin molecule, showing its orientation with respect to the crystal axes. Y is the diad axis. The small disk underneath represents a haem group drawn on the same scale and its correct orientation with respect to the crystal axes. The four lines on the cylinder surface indicate the positions of the concentrations of scattering matter deduced from the Fourier projections. The directions of the principal refractive indices are indicated by arrows. [This is merely a simplified diagramatic picture, giving as it were the fuzzy outline of the molecule. (From J. Boyes-Watson, E. Davidson, and M. F. Perutz, "An X-ray Study of Horse Methaemoglobin, I," *Proceedings of the Royal Society* A, 191 [1947], 123.)]

research degree, entered the little MRC unit of the Cavendish Laboratory, he taught himself the subject and read through the early papers on the structure of hemoglobin. After a year he gave a seminar on his conclusions which he titled, on the advice of Perutz's co-worker John Kendrew, "What Mad Pursuit." Perutz, Kendrew, and Bragg listened while this newcomer exposed the inadequacies of their techniques and attacked their picture of the hemoglobin molecule. Those were the days of the "hat-box" model (see Figure 1) in which the polypeptide chains were stacked in four layers all in parallel array like a neat pile of logs. This was consistent with the over-all electron density as calculated by Perutz, which in a vertical section through the hat box showed regularly spaced peaks of high density where the chains had been sliced through, and in transverse section, parallel ribs of high density where the chains ran across the box.

Crick believed that the technique of counting vectors so far employed was too superficial, and since a three-dimensional structural analysis was prohibitively lengthy, he tried reducing this to a two-dimensional analysis in the direction of the rods. This exercise, which he completed some time after his seminar talk, revealed a tenfold discrepancy between the model and the diffraction data. Crick concluded that only half the protein in the molecule could be arranged in the manner of the hat-box model. He believed there were more kinks, shorter straight runs, and that even when broadly parallel the chains meandered. By the time his thesis was completed, most of these con-

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clusions had been found less serious than he had thought at the time, but they did serve to jolt his colleagues out of a rut in which they had been working. His criticism and the subsequent analysis by Bragg marked the end of the belief in regular geometric structures for the globular proteins, a belief which had hitherto been widespread among protein crystallographers. Had crystallographers thought otherwise in those early days, they would never have attacked the proteins when they did. We are not to judge them for their oversimplified picture, for as Crick remarked: "It is one of the occupational hazards of the sort of crystallography in which you do not get results within a reasonable time, that those who work in it tend to deceive themselves after a bit; they get hold of an idea or an interpretation and unless there is someone there to knock it out of them, they go on along those lines, and I think that was the state of the subject when I went to the Cavendish."[26]

As we know from J. D. Watson's book *The Double Helix*, Crick's twenty-minute report on the techniques of structure analysis of proteins did not endear him to Sir Lawrence Bragg, although Kendrew and Perutz took it "remarkably well." The subsequent argument which Crick had with Sir Lawrence over priority for the "wave idea" exacerbated their relationship further. Nor could Crick settle down to one problem and see it through. Under Perutz's supervision he was supposed to find a protein smaller than hemoglobin and work out its structure. He considered secretin (an intestinal hormone which stimulates secretion of the pancreatic juice), which is about one-tenth the size of hemoglobin, but David Keilin, director of another Cambridge laboratory—the Molteno Institute—advised Perutz that this would be too difficult for a beginner to assay; Crick would have had to prepare it himself. So he turned to trypsin inhibitor. With this protein he was more successful and got as far as finding out the number of molecules in the unit cell. "What I should have done now, I realize pretty clearly," Crick said later, "is to have gone and looked at another species or done different salt conditions, but it was so discouraging, the number of molecules in the unit cell being so large, that I turned from it."[27] This was not just the impatience of a theoretician with the slow grind of experimental work, but the exasperation that many protein X-ray crystallographers feel from time to time.

As Crick turned from one trying topic to another, his thesis, "Polypeptides and Proteins: X-Ray Studies," became more and more of a "ragbag," and he considers it a miracle that he was able to make any thesis at all out of the material. On the other hand, there was in the pocket at the back of the thesis enough published material to make another one, "including of course the structure of DNA!"[28] In Great Britain it is the custom to submit published papers as accessory ma-

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terial; the reasons for excluding the work on DNA from the thesis itself were that "it would have meant recasting the whole plan of the dissertation and this would not have been easy at such a late stage. Secondly, the work was carried out in very intimate collaboration with Dr. Watson, and it would have been difficult to disentangle our respective contributions to it."[29]

It is surely also true to say that the section of his thesis dealing with the Fourier transform of a helix and the packing of helices in synthetic and natural polypeptides would have sufficed for a Ph.D. The Fourier transform of a helix was worked out in conjunction with the crystallographers V. Vand and William Cochran and predicted the characteristics of the diffraction patterns produced by continuous and discontinuous helices.[30] This theory enabled one to calculate rapidly the X-ray pattern predicted by a given model, a process which had hitherto consumed much time and which had normally been confined to the pattern along the equator (zero layer line) (see Figure 2). Bragg was mollified by this, the first evidence that Crick could see a good job through to a successful conclusion. By applying this theory to the early diffraction patterns obtained by W. T. Astbury, Crick was able for the first time to interpret them in terms of Pauling's α helices. Pauling had worked out the α helix for polypeptides while ill in London in 1948 but had refrained from publishing it because it did not account for the strongest feature of the diffraction pattern of keratin. Two years later he ventured a short note, because he had seen the patterns of synthetic polypeptides produced by the group at Courtaulds Ltd. in London and had found that they lacked the anomalous spot of α keratin.[31] His detailed papers followed in 1951.[32] In 1952 Crick was able to show that if the helices have a second twist of a higher order imposed upon them the keratin spot can be accounted for.[33] The same solution was arrived at independently by Pauling.[34]

The importance of the coiled-coil idea should not be underestimated, although it is still not unequivocally proven. The anomalous spot on the α keratin diagram had for years been the major stumbling block to any models so far advanced, including Pauling's α helix. Crick's coiled coil, which thus cut the Gordian knot for the students of proteins, is a good example of that physical intuition which enables Crick to brush aside the complexities of the situation and the plethora of possible alternatives and to concentrate on finding a simple physical model. The problem had been to account for the discrepancy between the meridional spot of 5.15 Å (angstroms) on the X-ray diagram of α keratin, and the 5.4 Å repeat of the Pauling-Corey helix. Knowing how difficult it would be for such a helix to pack down more tightly than the 5.4 Å pitch, Crick suggested that the helix is tilted in such a

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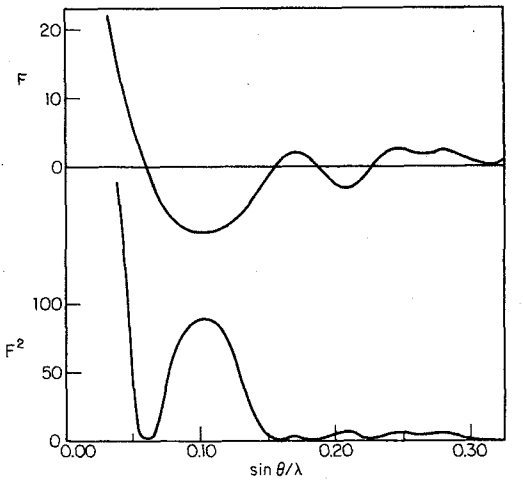

Figure 2a. The calculated X-ray form factor F and its square F^2 for equatorial reflections of nucleic acid. [Pauling's representation of the distribution of electron density moving out from the center of the X-ray diagram of DNA along the equator. (From L. Pauling and R. B. Corey, "A Proposed Structure for the Nucleic Acids," *Proceedings of the National Academy of Sciences,* 39 [1953], 94.)]

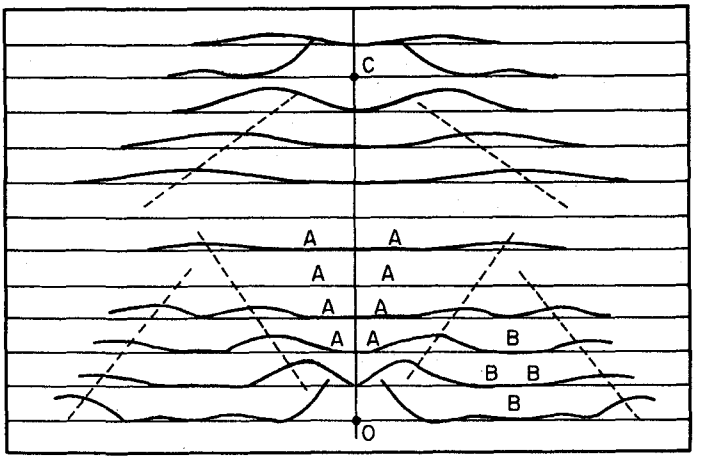

Figure 2b. Diffraction pattern of system of helices corresponding to structure of deoxypentose nucleic acid. The squares of Bessel functions are plotted about O on the equator and on the first, second, third, and fifth layer lines for half of the nucleotide mass at 20 Å diameter and remainder distributed along a radius, the mass at a given radius being proportional to the radius. About C on the tenth layer line similar functions are plotted for an outer diameter of 12 Å. [The more detailed picture obtained by Wilkins, Stokes, and Wilson for DNA using the helical transform theory. (From M. H. F. Wilkins, A. R. Stokes, and H. R. Wilson, "Molecular Structure of Deoxypentose Nucleic Acids," *Nature,* 171 [1953], 38.)]

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way as to split the single meridional 5.4 Å reflection into two near-meridional spots at 5.1 Å. Simply by bending the helix into a coiled coil with a pitch angle of 18° he was able to generate the required spots, for 5.4 Å × cos 18° = 5.1 Å.

When Crick met Pauling in the summer of 1952, the structure of α keratin was mentioned, and they agreed that the solution may involve the coiling of α helices around one another. The details were not discussed because their analyses were still unsatisfactory, but by the autumn both men felt more confident. The result was that the editorial office of *Nature* received almost simultaneously a paper from Crick and one from Pauling and Corey on the solution of the same problem. Crick's paper, dated October 22, 1952, appeared in the November 22 issue; Pauling and Corey's, dated October 14, appeared in the January 10, 1953, issue. Unfortunately, the papers were not treated in strict order of receipt; it just happened that Pauling's paper had three diagrams which necessitated the making of blocks, Crick's paper had none and was supported by a letter from Bragg. A comparison of the dates of other papers in the January 10 issue of *Nature* shows that Pauling's paper was not delayed, but that Crick's paper was pushed ahead.

Leaving the unfruitful topic of priority to one side, a reading of both papers does bring out some of the differences between the authors' approaches and style of writing. Crick's paper is easier to read and introduces the "nobs and holes" concept for interleaving the side chains. He gives an approximate calculation of the force necessary to deform the helix into the coiled coil. Pauling, on the other hand, does a comparison of the observed and calculated X-ray intensities along the equator and postulates the packing of the helices into three- and seven-stranded ropes, which he illustrates. Whereas Pauling favored a super-helix pitch of 66 Å, Crick suggested 198 Å. Support for the latter alternative may be found in the recent discovery of two near-meridional spots on the α keratin pattern of mohair by Spei, Heidemann, and Zahn.[35]

The Structure of DNA

This background of helical expertise and detailed knowledge of the crystallography of hemoglobin served Crick well when he collaborated with Watson on DNA. The first time that Crick studied the literature on DNA was when he gave a talk about it at Strangeways Laboratory in the 1940's. All he can now remember of that event was that he noted the way in which radiation reduced viscosity. Clearly he had no grounds for regarding DNA as being as important as proteins or more so, and since single crystals of DNA cannot be produced, no

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crystallographer in his right mind would tackle it save for very compelling reasons. Indeed he often teased his friend Maurice Wilkins, of King's College, London, about the difficulties Wilkins was having with DNA, and once in the Embankment Gardens over tea he quipped, "What you ought to do is get yourself a good protein."[36] About this time Crick was present at a small protein meeting in Cambridge on July 6, 1950, at which Wilkins gave an account of his work.[37] Wilkins showed his early X-ray pictures and pointed out the correlation between the inclination of the "cross-ways" pattern and the slope of a helical or zig-zag molecule. Although he is sure he was present for the whole of the meeting, Crick cannot recall anything about Wilkins' lecture. Wilkins' former collaborator, William Seeds, does not find this surprising since he can distinctly remember that on more than one occasion Crick was seen to be in active conversation at the back of the hall.[38]

Although Crick was not aware of the overriding importance of DNA in 1950, he had immersed himself in biology to the point of developing interests outside the sphere of structural molecular biology to which the Cavendish group belonged. Thus it came about that when Watson came to Cambridge in the fall of 1951 Crick found him to be the first person who thought the same way as he did about biology. True, the X-ray crystallographers were working on biologically important molecules, and so were the biochemists; the geneticists, too, were hard at work on the fine-structure analysis of the genetic map. But Crick wanted to find the link between gene structure and protein structure. The specificity of proteins must be based on the unique character of their amino acid sequences and these must be related to the sequential arrangement of the hereditary material. In Watson he found a biologist steeped in genetics, anxious to discover how the gene works at the molecular level.

In a letter to Max Delbrück, Watson wrote of Crick: "He is no doubt the brightest person I have ever worked with and the nearest approach to Pauling I've ever seen—in fact he looks considerably like Pauling. He never stops talking or thinking, and since I spend much of my time in his house (he has a very charming French wife[39] who is an excellent cook) I find myself in a state of suspended animation. Francis has attracted around him most of the interesting young scientists and so at tea at his house I'm liable to meet many of the Cambridge characters."[40]

The circumstances which led Watson and Crick to work on DNA are too well known by now to need detailing, but the course of the scientific argument deserves repeating.[41] As Watson has recounted, the first of their two attempts at the structure was vitiated at the start by

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Watson's faulty report of the data presented by Rosalind Franklin (who was working with Wilkins at King's College) in November 1951 when he mistook eight molecules of water per lattice point,[42] for eight per unit cell, thus making the sodium salt of DNA appear to have twenty-four times less water than Franklin had said. Crick admits that had he known more chemistry he would have realized that this figure is impossible, since the sodium ions must have water shells around them. With these shells, positively and negatively charged atoms or groups can be at a distance from one another in the crystal; without them, charged groups must lie close together. Since the only source of these is the phosphate group, clearly the metal ions must lie against the sugar-phosphate backbones of the DNA chains. Now from Astbury's density measurements it was known that in the molecules, more than one chain is present—probably three; what better means to hold these chains together than electrostatic attraction between the charged phosphate groups and the metal ions? In her seminar talk Franklin had used such a scheme to hold *neighboring* molecular chains together in the crystallites of the *A* form of DNA, and she had explained the loss of crystallinity incident on the addition of water which formed the *B* structure of DNA as due to the destruction of the sodium-phosphate linkages by interpenetration with water molecules. This scheme rested on her conviction that the sugar-phosphate backbones were on the outside of the molecule.[43] Watson and Crick reversed her argument, putting the backbones on the inside of the molecule and using the phosphate-sodium attractions as the force accounting for the known rigidity of the DNA molecule (see Figure 3). Whether in his report Watson confused inter- with intra-molecular attraction is unclear. What is clear is that Watson and Crick were convinced that electrostatic attraction between the metal ions and the phosphate groups was greater than any hydrogen bond formation, since only the bases could be active in this way.

Thus in an unpublished manuscript of late 1951, Crick wrote: "There are no atoms which can donate hydrogen bonds except in the basic rings and the water. Thus hydrogen bonding is unlikely to play the dominant part in the structure that it does in the polypeptide α helix. Electrostatic forces are so big relative to Van der Waals Forces that we may be confident that the Na^+ and the PO_2^- will mainly decide the arrangement."[44] Furthermore, there appeared to be evidence of variations in the structure of the bases such that precisely directed hydrogen-bond formation would be impossible. All in all, the case for a hydrogen-bonded structure like Pauling's α helix appeared weak.

The fiasco over the resulting three-stranded model is well known. Rosalind Franklin quickly spotted the error over the water content—there were only eight water molecules per twenty-four bases—and rightly

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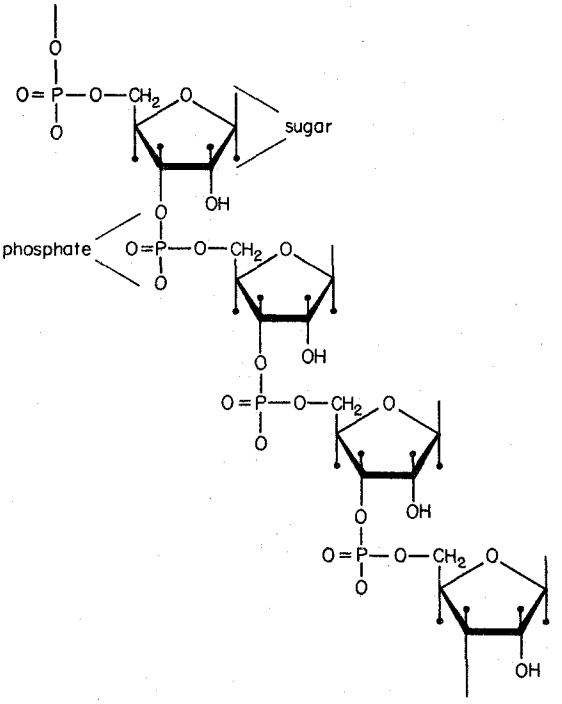

Figure 3a. Detail of Sugar-Phosphate backbone.

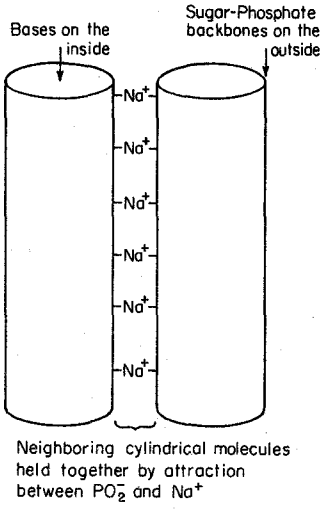
Bases on the inside
Sugar-Phosphate backbones on the outside
Na+
Neighboring cylindrical molecules held together by attraction between PO2- and Na+

Figure 3b. Franklin's scheme in 1951 (not drawn to scale).

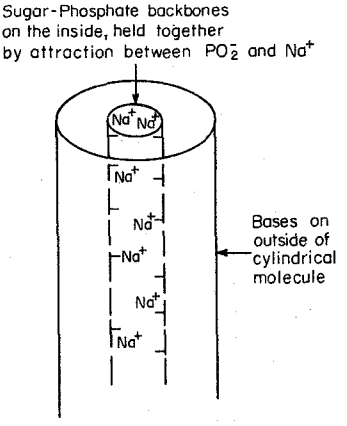
Sugar-Phosphate backbones on the inside, held together by attraction between PO2- and Na+
Na+
Bases on outside of cylindrical molecule

Figure 3c. Watson and Crick's scheme in 1951 (not drawn to scale).

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insisted that the sugar-phosphate backbones must be on the outside, not the inside. Crick returned to his protein studies for his thesis and Watson turned to the other form of nucleic acid—RNA—in the tobacco mosaic virus. The paper, "The Structure of Sodium Thymonucleate: A Possible Approach," which Crick had written, along with the trial nets showing the topology of the phosphate-sodium spacings went back into his DNA file and was never published. But DNA continued to nag. Watson got very interested in the possible role of divalent ions in holding the chains in DNA together and had bacteriophage DNA from Copenhagen tested in the flame photometer for the presence of small quantities of magnesium.[45] Perhaps there was just enough magnesium in the sodium salts examined by Wilkins and Franklin to act as bridges between the sugar-phosphate backbones in the molecule.

This approach to finding the attractive forces in DNA did not appeal to Crick. Instead, he considered the electrostatic attraction between dipoles produced by a net shift of charge over the bases (now termed stacking forces). Through Professor Kemmer he was introduced to a young Cambridge mathematician, John Griffith, the nephew of Frederick Griffith, who discovered bacterial transformation *in vivo*. Griffith agreed to calculate what attractive forces there would be between *like* bases. That Crick had in mind attraction along the direction of the chains and not *across* from one to another is evident from his recollection of the event in which he said: "Since the bases are flat, perhaps that is so that they can stack on top of one another and attract. Why not work out if adenine attracts adenine, and so on?"[46] The idea was, of course, that the bases belonging to different chains of the same molecule are interleaved. (It is interesting to note here that in 1946 K. G. Stern, working at Yale, advanced a model with hydrogen bonding between amino and keto groups of interleaved bases.[47])

Later, Crick spotted Griffith in the tea queue and asked "Have you done the calculations?" Griffith said, "Yes, and I find that adenine attracts thymine, and guanine attracts cytosine." Crick replied, "Well, that is all right . . . *A* goes to make *B* and *B* goes to make *A;* you just have complementary replication." At this stage Crick had realized, and Griffith too, that preferential attraction between adenine and thymine and between guanine and cytosine would give rise to complementary replication. Neither had yet read Pauling and Delbrück's 1940 paper stating a preference for this type of replication rather than like-with-like schemes, but Griffith had already, unknown to Crick, worked out a scheme of hydrogen-bonded complementary base pairs (pyrimidine with pyrimidine and purine with purine) for nucleic acids, which he never published. That he did not mention this to Crick is understandable, for he was a young mathematics graduate taking an undergraduate course

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in biochemistry, introverted and unassuming; whereas Crick was older, extroverted, and in Griffith's eyes an established scientist. As Crick's questions all related to stacking pairing schemes, not planar schemes, there was no purpose in Griffith's bringing up his hydrogen-bonded scheme[48] (see Figure 4). Griffith had become interested in biochemical genetics from taking the Part II biochemistry course in the Cambridge Tripos in 1950-1951. It was probably around the spring of 1951 that he began to puzzle over gene replication. At first he had considered amino-acid matching for protein replication, but on learning about the possible role of nucleic acids in the process, he considered base-pairing schemes. From what source he learned of the hereditary role of DNA he cannot now recall, though he thinks that he probably read a review article.

Shortly after Crick's meeting with John Griffith in the tea queue, John Kendrew introduced Watson and Crick to the noted nucleic acid biochemist Erwin Chargaff, of the College of Physicians and Surgeons, Columbia University. Crick recalls the occasion as follows: "We were saying to him as protein boys, 'Well, what has all this work on nucleic acids led to? It has not told us anything we want to know.' Chargaff, slightly on the defensive, replied: 'Well, of course there are the 1:1 ratios.' So I said: 'What is that?' And he said: 'Well, it is all published!'

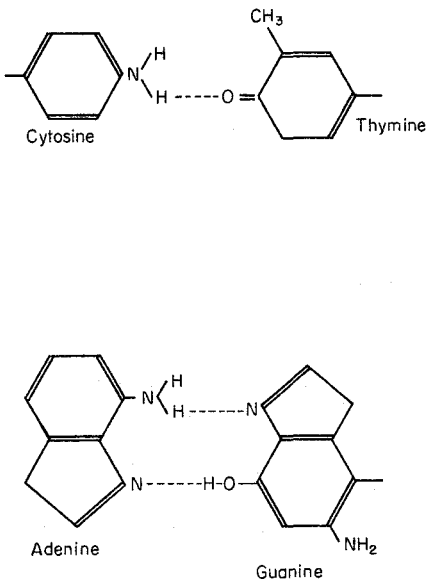

Figure 4. Probable nature of Griffith's schemes for base-pairing in 1951.

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Of course I had never read the literature, so I would not know. Then he told me and the effect was electric. That is why I remember it. I suddenly thought: 'Why, my God, if you have complementary pairing, you are bound to get a 1:1 ratio.' [See Table 2.] By this stage I had forgotten exactly what Griffith had told me. I did not remember the names of the bases. Then I went to see Griffith and asked him which his bases were and wrote them down. Then I had forgotten what Chargaff had told me so I had to go back and look at the literature. And to my astonishment the pairs that Griffith said were the pairs that Chargaff said."[49] Hence at this stage, June 1952, Crick realized that base pairing (although the wrong sort) could be the cause of the Chargaff rules.

This simple inference which had escaped everyone except G. R. Wyatt (who first thought of it in March 1953)[50] is rather like the explanation of Mendelian ratios in terms of germinal segregation. Of the so-called three rediscoverers and the several other plant breeders who published such ratios in 1900, there are good grounds for believing that only one established the connection: Carl Correns. What seems an obvious step with the advantage of hindsight is rarely so without it. Whether Crick would have realized that Chargaff's ratios meant base pairing had he not known the outcome of Griffith's calculations cannot be decided, but Griffith's work made sure he did.

The strange aspect of this story is the fact that Griffith was able to arrive at the correct pairs of bases using calculations which he knew and which were generally known to be too approximate to give reliable answers except for the simplest of compounds. It is clear that he examined unlike pairs because of the likelihood that replication is complementary rather than identical. On the other hand, he would hardly have omitted telling Crick about Chargaff's data had he known it at

Table 2. Composition of two microbial desoxyribonucleic acids.*

Constituent	Yeast		Avian tubercle bacilli
	Prep. 1	Prep. 2	
Adenine	0.24	0.30	0.12
Guanine	0.14	0.18	0.28
Cytosine	0.13	0.15	0.26
Thymine	0.25	0.29	0.11
Recovery	0.76	0.92	0.77

* These figures show that the ratio of *A* to *T* and of *G* to *C* are close to 1. (From E. Chargaff, "Chemical Specificity of Nucleic Acids and Mechanism of Their Enzymatic Degradation," *Experientia*, 6 [1950], 205.)

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this stage. The correlation would have been so exciting as to cause the quiet and calm young Griffith to proclaim it. The correct pairing must have been a fluke. At the time, Griffith told Watson and Crick that π orbital interaction might account for the adenine-thymine and guanine-cytosine preferences. In fact, specific pairing is brought about not by electrostatic attraction in the stacking direction—though such forces help to hold the molecule together—but by hydrogen bonding perpendicular to the fiber axis. *The Double Helix*, according to Crick, "gives the impression that we knew all about Chargaff's rules before we talked to John Griffith. Now that may be formally so, but it does not correspond with what was actually going on in our minds."[51]

This brings us to an even stranger episode in the story. Watson and Crick were now fully aware of Chargaff's rules and—on the basis of Griffith's calculations—of the possibility that a structural relationship between pairs of bases might explain them. Why did they not at once set to work to build a model incorporating base pairing? Here it is easy to telescope events and to imagine that they had in mind hydrogen-bonded base pairing in the plane of the bases, whereas in fact not once in 1952 was any attempt made to figure out such schemes, for they were thinking in terms of stacking pairing of interleaved bases. Hydrogen bonding was, they thought, too unreliable as a mechanism of replication. Here Crick made the mistake of assuming that both tautomeric forms of the bases exist in the same DNA molecule, that the proton could shift from one position to another, thus altering the sites for hydrogen bond formation. Now if these sites varied in the same base, how could one get constancy of matching in replication? Crick was not alone in holding this view, for L. Hunter had described the tautomeric distinction in certain cases as "meaningless"[52] and P. G. Owston got evidence of the easy migration of the proton in ice crystals.[53] Coming nearer home, June Broomhead, in her Cambridge Ph.D. thesis on the structure of purines, had found a curious electron density distribution near the amino group, reminiscent of Hunter's proton migration effect.[54] To cap it all, textbooks figured here one tautomeric form of the bases, and there another.[55]

Stacking pairing schemes also called for caution: both Crick and Griffith knew how unreliable are conclusions based on quantum mechanical arguments. Further, the Chargaff rules might yet prove to be a red herring; they might be the product of some feature of the code or of the metabolism of the bases. Crick hated using more than the minimum number of assumptions, for by so doing, it would be easy to exclude the correct approach. They had yet to find some direct evidence for base pairing.

At the end of July 1952 Crick tried to get such evidence by determining the ultraviolet absorption of mixed and pure bases. If the mixtures

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entered into pairing relationships, they would absorb less ultraviolet radiation than would the sum of their constituents irradiated separately. These experiments failed to show any difference, because Crick lacked small enough cells to achieve the required concentrations without bringing on opacity, and as he was in the last year of his thesis, he dropped this unpromising attempt. But he continued to feel the need for more evidence.

The trouble with X-ray crystallography is that there are only two ways to attack a structural problem. The conventional method is to use single crystals which will give sufficient data for an unambiguous structure determination providing that the phase problem can be solved. Alternatively, one can attempt a solution on the basis of slender data derived from powder or fiber diagrams, making the minimum number of assumptions and developing the argument in a systematic manner—a procedure resembling a logical argument in that if the premises are wrong the whole argument will be wrong. The Cavendish group had earlier failed to discover the α helix because they believed that the amide bond is not planar and they only allowed for integral screw axes in their models. The resulting fiasco in 1950 left its mark on all in the unit.[56] Bragg regarded their paper on the possible polypeptide helices as the worst they had ever written. Crick was determined not to slip up like this over DNA. The unpublished 1951 structure had been bad enough.

A further attempt on DNA was not made until 1953 and then only because Pauling had produced a structure shortly to be published.[57] As for the relative positions of Watson and Crick in this second attempt, it is clear that to sharply divide the roles into Crick the crystallographer and Watson the geneticist is not justified by the facts. The suggestion that replication of DNA is complementary, not identical, arose out of Griffith's calculation which Crick connected with Chargaff's ratios. The majority of geneticists were by no means sold on complementary replication, and their hero—H. J. Muller of Indiana University, where Watson studied as a graduate—had favored a like-with-like scheme. Either scheme suggested two chains, as did analogy with chromosomes, which sufficed to convince Watson; but Crick feared there might be a third chain—as indeed the density at that time suggested—for transmitting information to the cytoplasm.

Evidence *so far collected* suggests that this successful attempt in 1953 to determine the structure of DNA took from Friday, February 6, when Watson took the Pauling DNA manuscript with him to King's College, London, until Saturday, February 28, when Crick retired to bed exhausted after nearly a week of model building. At King's, Watson learned from Wilkins that the density data did not after all rule

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out two-chain models, and that the sugar-phosphate chains must, as Franklin had stated in Watson's presence in 1951,[58] be on the outside. At the end of the first week back in Cambridge, Watson had come round to this view. Crick recalls an earlier incident when Watson was complaining of the difficulties he was having in the attempt to intertwine the backbones on the inside of the structure. Crick's reaction was to ask: "Then why don't you put them on the outside?" Watson replied: "That would be too easy," which prompted the retort: "Then why don't you do it!"[59] Watson himself wrote: "Finally over coffee I admitted that my reluctance to place the bases inside partially arose from the suspicion that it would be possible to build an almost infinite number of models of this type. Then we would have the impossible task of deciding whether one was right. But the real stumbling block was the bases. As long as they were outside, we did not have to consider them. If they were pushed inside, the frightful problem existed of how to pack together two or more chains with irregular sequences of bases. Here, Francis had to admit that he saw not the slightest ray of light."[60]

At the same time, Watson's building of sugar-phosphate backbones had been vitiated by the narrow separation of the sugars. "Watson," said Crick, "was always trying to build it with the sugars too close. He went away one day and said: 'You try it.' and while he was playing tennis I didn't try his scheme; I was convinced it was wrong. Instead, I built one with a 36° rotation."[61] In the following week, while Crick continued to work on his thesis, Watson either played tennis or tried to figure out a scheme for packing the bases. On Thursday, February 19, he arrived at a like-with-like pairing scheme which Jerry Donohue (an American crystallographer who shared Watson and Crick's office) and Crick discredited on the following morning,[62] Donohue on the grounds that he had used the wrong tautomeric forms, Crick because the axis of symmetry of the molecule was in the wrong direction and the structure did not explain Chargaff's rules.

"The crucial point," said Crick, "was when we asked Donohue about the tautomeric forms, because when he put us right we could put the hydrogen bonds together. At that stage, and I remember this very clearly, Jerry and Jim were by the blackboard and I was by my desk, and we suddenly thought, 'Well perhaps we could explain 1:1 ratios by pairing the bases.' It seemed too good to be true. So at this point [Friday, February 20] all three of us were in possession of the idea we should put the bases together and do the hydrogen bonding."[63] At last the misjudgment over tautomeric shifts had been disposed of, so Crick as well as Watson could be enthusiastic about hydrogen-bonded base pairs; and they had been given an authoritative decision as to which tautomers are present in DNA.

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Using the cardboard replicas of the DNA bases which he had cut out the previous afternoon, Watson arrived at the correct scheme on Saturday, February 21, but not by consciously trying to apply Chargaff's rules. He said: "Though I initially went back to my like-with-like prejudices, I saw all too well that they led nowhere. When Jerry came in I looked up, saw that it was not Francis, and began shifting the bases in and out of various other pairing possibilities. Suddenly I became aware that an adenine-thymine pair held together by two hydrogen bonds was identical in shape to a guanine-cytosine pair."[64] Both Donohue and Crick quickly gave their blessing to Watson's base pairs, and Crick pointed out that since the axis of symmetry of the unit cell was perpendicular to the fiber axis, the sugar-phosphate backbones must run in opposite directions, as indeed the direction of the links between base and backbone indicated in Watson's pairs. This meant that both purines and pyrimidines can be accommodated on the same chain without a change in the orientation of the glucosidic links. At this stage, the chief features of the *in vivo* form of DNA were discovered. Two weeks had elapsed since Watson's visit to London; a further week was required to build the model incorporating these features and to measure its parameters accurately.

The model building now was done not on several residues and base pairs, but on a single residue "and the appropriate base." Crick continued, "I worked out a theorem which now escapes me which expressed the geometrical restraint of the other base on this one. I knew that as long as I obeyed this geometrical rule, some projection of something on something else, one could do it . . . The model building was done in a period which I remember rather vividly. It started about Wednesday and finished on a Saturday evening [February 28], by which time I was so tired, I just went straight home and to bed."[65] Those three weeks of model building and discussion were marked by a suppressed excitement which became intense. They could talk and think of nothing else. Not even Hedy Lamarr's romps in *Ecstasy* could take Watson's mind off the bases.[66]

Watson's story of the discovery of the double helix differs from Crick's, for the obvious reason that what was going on in Watson's mind at the time differed from what was going on in Crick's mind. Crick had made a major error in thinking that the bases would suffer tautomeric changes sufficiently often to make specific hydrogen bonding impossible. It says much for Watson's strength of character that he did not allow Crick's opinion to deter him in the end from examining hydrogen-bonded schemes. So it was Watson who benefited from J. M. Gulland's remarks about hydrogen bonding[67] and Broomhead's patterns of hydrogen-bonded molecules in purine crystals. So Watson

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thought more about hydrogen bonding than did Crick. On the other hand, Watson did not appear to have understood the arguments which Crick developed from the knowledge that DNA crystallites belong to the space group C2, hence he continued to try to get a like-with-like pairing scheme after Crick had told him it would not square with the crystallographic evidence. In the collaboration they compensated for each other's blind spots. But their collaboration involved far more than this. Alexander Humboldt expressed the essence of such work well when he said: "Collaboration operates through a process in which the successful intellectual achievements of one person arouse the intellectual passions and enthusiasms of others, and through the fact that what was at first expressed only by one individual becomes a common intellectual possession instead of fading away into isolation."[68] At first, Watson did the work of model building and Crick interrupted his thesis writing from time to time only to advise him, but after base pairing had been discovered, Crick threw himself into model building on a full-time basis.

One other feature of the story deserves underlining. Because of the nature of the problem, biological arguments seem to have played a very minor role. It is true that Watson ruled out three-chain models for biological reasons, but not before Wilkins told him that the physical data on DNA did not exclude two-chain models. Second, Watson did not study base-pairing schemes until forced to do so by the physico-chemical evidence that the sugar-phosphate chains must be on the outside.

The next four weeks were occupied with writing draft after draft of the now famous *Nature* paper,[69] showing visitors the model, and awaiting the parallel contributions from the King's group. Then there was a second *Nature* paper written chiefly by Crick, and a paper for *Discovery* by Crick. In June the Cavendish had an open day for members of the University, for which Watson and Crick built their model from the table almost to the ceiling of their office. Watson wrote and presented a paper for a symposium held at the Cold Spring Harbor Laboratory in Long Island, New York, and he wrote the major part of a paper for the *Proceedings of the Royal Society*. On the order of names, they decided that since Watson had been closely associated with most of the work, his name should appear first on the April 25 *Nature* paper. The second *Nature* paper, written chiefly by Crick, was decided by the toss of a coin which Watson won. The order for the Cold Spring Harbor paper was decided by the fact that Watson was the one invited to give the paper, and for the Royal Society paper, custom dictated the alphabetic rule.[70]

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The Genetic Code

In the summer of 1953 the cosmologist George Gamow sent Crick a copy of his short note to *Nature* which marked the beginning of work on the genetic code using the Watson-Crick model. Watson and Crick could say in 1953 about the genetical implications of the DNA structure that it offered a mechanism of replication. They wrote (in their second 1953 paper to *Nature*): "If the actual order of the bases on one of the pair of chains were given, one could write down the exact order of the bases on the other one, because of the specific pairing. Thus one chain is, as it were, the complement of the other, and it is this feature which suggests how the deoxyribonucleic acid molecule might duplicate itself."[71] They suggested that tautomeric shifts during the replication process might account for mutations, and with regard to protein synthesis they emphasized that the "open" nature of their structure meant that the bases, though "inside," are sufficiently accessible to allow DNA to exert its "highly specific influence on the cell."[72] Although at this stage they had the sequence hypothesis clearly in mind, they were very careful not to suggest how the information in the base sequence might be expressed. (This will be discussed later.)

Gamow forced the issue over the genetic code by presenting his scheme of geometric specificity of the "holes" in the DNA molecule. When Crick received from Gamow the text of the *Nature* paper[73] and the expanded version intended for the *Proceedings of the National Academy of Sciences*[74] which contained the list of Gamow's twenty standard amino acids, he sat down with Watson in the Eagle Hotel, where they regularly had lunch, and wrote out the correct list. Gamow's choice of twenty indicates that his knowledge of protein chemistry was not quite adequate for the task: While he excluded common amino acids like asparagine and glutamine, he included the rare amino acid hydroxyproline (see Table 3). He was also unaware that protein synthesis takes place almost exclusively in the cytoplasm (there was some evidence at the time suggesting that it also occurs in the nucleus, but it is doubtful that he knew about this). Nevertheless, although he was wrong Gamow proved a great stimulus. His DNA scheme could be applied equally to RNA in the cytoplasm, and it did contain the idea of degeneracy and triplet codons, as did the 1952 scheme of Alexander Dounce.[75]

Crick took up the subject of the code in the autumn of 1953. The way he handled it illustrates his deductive approach—reasoning from the minimum of assumptions and seeing where the argument would lead. Deductive though he is, he always seeks some hard evidence

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Table 3. The twenty amino acids in protein synthesis.

Gamow's list	Watson and Crick's list
Glycine	Glycine
Alanine	Alanine
Valine	Valine
Leucine	Leucine
Isoleucine	Isoleucine
Proline	Proline
Phenylalanine	Phenylalanine
Tyrosine	Tyrosine
Serine	Serine
Threonine	Threonine
	Asparagine
	Glutamine
Aspartic Acid	Aspartic Acid
Glutamic Acid	Glutamic Acid
Arginine	Arginine
Lysine	Lysine
Histidine	Histidine
Tryptophane	Tryptophane
	Cysteine
Methionine	Methionine
Cystine	
Cysteic Acid	
Hydroxyproline	

before he will publish his conclusions. "I have always had great difficulty," he said, "in publishing theoretical ideas in a vacuum of evidence." One has to be cautious about publishing ideas, "partly because if you are a professional theoretician as I am, you cannot afford to publish too many wrong ones."[76] So later when Gamow, whom he had first met at Woods Hole in the summer of 1954, asked him to join his RNA Tie Club,[77] he was delighted to accept. Members of the club could circulate papers for discussion without publishing them: it was a sort of preprint club. Hence the adaptor hypothesis, which Crick put forward at the Gordon Conference in June 1956, he had first described in a paper for the RNA Tie Club. The "comma-less code" was also presented first to the club and published only because "three or four people wanted to quote it."[78] At the time Crick was very guarded about its importance—which turned out to be an appropriate attitude, for it was quite wrong.

Initially, Crick, like most other molecular biologists, did not perceive

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the weakness in Gamow's scheme. In 1954 interest in the "holes in RNA" was intense. Watson, Delbrück, and Rich were studying the holes in a hypothetical model of RNA in Pasadena, and Gamow, who visited them, wrote to Crick: "They have a model of RNA, big and nice looking, but they do not believe in it very much themselves (except Alex Rich, who conceived it). It has trapezoid holes formed by two bases, and two different 'sugar edges.' And there are twenty different holes. But I have found . . . that the combination rules do not work at all."[79] In fact, Crick had tested Gamow's scheme two months earlier while he was at the Brooklyn Polytechnic Institute by applying it to Frederick Sanger's data on insulin, and he found it demanded a greater correlation of neighboring amino acids than insulin possessed. By the summer of 1954, he was convinced that holes in the helix schemes were all wrong. In his RNA Tie Club paper "On Degenerate Templates and the Adaptor Hypothesis," Crick wrote: "I don't think that anybody looking at DNA or RNA would think of them as templates for amino acids were it not for other indirect evidence. What the DNA structure *does* show (and probably RNA will do the same) is a specific pattern of *hydrogen bonds*, and very little else. It seems to me, therefore, that we should widen our thinking to embrace this obvious fact."[80]

Crick went on to postulate adaptor molecules composed either of amino-sugars or of di-, tri-, or tetranucleotides to which specific enzymes could attach the right amino acid; the adaptor thus charged would then pair specifically with the codons in RNA.[81] At the time when Crick's adaptor hypothesis was still unproven, Paul Zamecnik's group at the Massachusetts General Hospital in Boston was studying the incorporation of labeled RNA precursors in the "cell-free system." They had been puzzled to find some radioactive counts in the RNA fraction of the control experiment to which radioactive amino acids had been fed, as well as in the system supplied with radioactive nucleotides. It was just possible that some RNA species might be present in the cell-free system to which the radioactive amino acids became attached. They quickly established this point and Mahlon Hoagland, who had come to work with Zamecnik on protein synthesis, identified the fraction as the nonribosomal RNA "which up to that time, had not been of interest to anyone and had been discarded as 'soluble RNA' . . . possibly a breakdown product of ribosomes!" Zamecnik's group went on to show that this substance is an intermediate in protein synthesis and, according to M. B. Hoagland, "published all this work in early 1957, *with no knowledge of Francis' hypothesis*! Jim Watson first told me of the adaptor hypothesis about Christmas 1956 and in January 1957 Francis visited us, enormously excited, and we quickly arranged that I spend a year with him in

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Cambridge [1957-1958], which I did!"[82] This story was recalled somewhat differently by Zamecnik in 1969 when he said:

> There were already in the air at this time [1955-1956] a few suggestions that something might be missing between the activation and the sequentialization steps . . . We had also found that a purified tryptophane activating enzyme . . . was inactive in promoting tryptophane incorporation into protein. A rumour had also reached us that at a recent meeting Francis Crick had expressed the opinion that there should be an oligonucleotide intermediate as a language translation piece between activated amino acid and RNA template.
>
> It was at this point that we prepared a ^{14}C aminoacyl RNA and Mahlon Hoagland plugged it into the cell-free system to determine whether it could substitute for free ^{14}C amino acid and serve as an intermediate.
>
> In retrospect, it may be asked why we allowed over a year to elapse before reporting on the existence of RNA, since from the first positive experiment in November 1955 we regarded this molecule as a possible new and dazzling transfer intermediate. We were, however, anxious to remove all doubts that (a) the amino acid was covalently bound, (b) that the aminoacyl nucleotide was a direct line intermediate in the pathway from free amino acid to peptide, and not a side path for storage of activated amino acids, and (c) that the entire complex polynucleotide—and not a much smaller triplet or oligomer which adventitiously aggregated with this larger type of RNA—was the active intermediate.[83]

Whatever the precise facts of the matter may have been, Hoagland in 1959 expressed the important point: "Here was one of those rare and exciting moments when theory and experiment snapped into soul-satisfying harmony."[84]

Because these soluble RNA molecules were larger than trinucleotides, Crick regarded them as "a half-way step in this process of breaking the RNA down to trinucleotides and joining on the amino acids."[85] He feared that if the adaptor molecules were as large as soluble RNA, there would not be enough space for them to function properly or they would take too long to get into the right positions. Crick and Hoagland were both embarrassed by the unexpectedly large size of soluble RNA species, and in their correspondence before Hoagland's arrival in Cambridge they discussed possible ways of getting around the problem.[86] Now it is recognized that the adaptor molecules are all large molecules.

Messenger RNA

Where Crick's thinking—and that of the majority of molecular biologists of the day—was completely at sea was over the function of the ribosomes, or "microsomal particles" as they used to be called. Crick describes this as the one big howler in molecular biology, for unlike the holes-in-the-helix idea, it lasted a long time—nearly seven years.

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From the early work of Jean Brachet and Tobjorn Caspersson it was known that protein synthesis is associated with cytoplasmic RNA. So it was concluded that for every protein, or perhaps for every polypeptide chain, one RNA molecule is assembled on the chromosomal DNA and passes out into the cytoplasm to form a stable, metabolically active particle (the ribosome) on which one species of protein is synthesized. A major rethink had to be undertaken before this idea was disposed of and a new RNA species—messenger RNA—introduced, the ribosomes being recognized as nonspecific sites for protein synthesis. Here the old ribosomal "template RNA" theory would not bend to the results of experiments on phage infection and ribosomal longevity.

Matters came to a head in the autumn of 1959, and for Crick and Brenner the turning point came in the spring of 1960 when two informal meetings were held in Cambridge to discuss the "Pa-Ja-Mo" experiment of Arthur Pardee, François Jacob, and Jacques Monod. In 1958 these three, working at the Pasteur Institute, had succeeded in conjugating bacteria to produce heterozygotes for the *lac* operon (the genetic region associated with utilization of lactose sugar) and they found, much to their surprise, that when a recipient mutant bacterium unable to synthesize the enzyme β-galactosidase received the "wild-type" gene (the typical form in a natural environment), synthesis of the protein started up within three to four minutes. It did not build up gradually, as one would expect if ribosomes acting as β-galactosidase templates had to be synthesized *de novo*. Perhaps, they suggested, the DNA introduced was acting as the template directly; or some DNA-like RNA had been transferred with the DNA; or there was some other short-lived RNA like the soluble RNAs but big enough to encode the protein, to act as messengers between DNA and the sites of protein synthesis (see Figure 5).[87]

When Jacob spoke at a conference in Copenhagen in the autumn of 1959 his audience, which included Crick, proved unsympathetic to the idea of an unstable RNA species—a "tape" or "X" as Jacob recalls they then termed it[88]—because the Pa-Ja-Mo experiment alone was not thought adequate to exclude other explanations. But by 1960 the situation had changed: experiments of Monica Riley supervised by Pardee at Berkeley showed that the destruction of the same gene for β-galactosidase synthesis by ^{32}P decay quickly depressed the rate of synthesis of this enzyme, and these experiments together with the earlier one ruled out ribosomes and any long-lived nucleic acid intermediate, and defined what the authors described as an "interesting dilemma."[89] Either the gene introduced acts directly, or there must be some functionally unstable intermediate involved in the information

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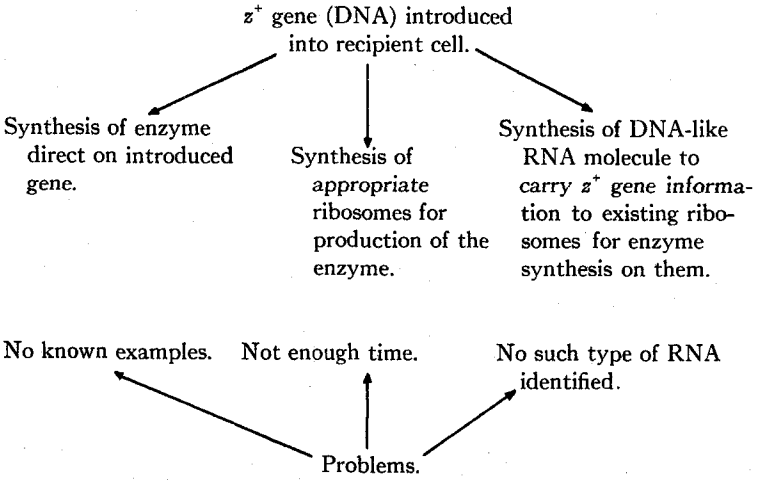
z^+ gene (DNA) introduced into recipient cell.
Synthesis of enzyme direct on introduced gene.
Synthesis of *appropriate* ribosomes for production of the enzyme.
Synthesis of DNA-like RNA molecule to *carry* z^+ *gene information* to existing ribosomes for enzyme synthesis on them.
No known examples.
Not enough time.
No such type of RNA identified.
Problems.

Figure 5. Possible explanations of the "Pa-Ja-Mo" experiment.

transfer from DNA to the site of protein synthesis. Unfortunately, there appeared to be no hard evidence for the presence of such a species of RNA, and the natural tendency was to try to find other explanations of the mechanism involved. Thus when Pardee had suggested "an intermediate carrier of information (perhaps RNA)" in 1958 he doubted how such a mechanism could be squared with the demonstration of constant rate of protein synthesis after mating: "a number of possible but not too plausible explanations exist based on the idea that the cell is saturated with templates in a few minutes, or that the number of templates reaches equilibrium in a few minutes, or that the template might be unstable and rapidly reach a steady state concentration as a balance of formation and inactivation [now accepted as the correct explanation]."[90]

When the Pa-Ja-Mo experiment was first performed in 1958 the sudden switching on of protein synthesis caused Pardee, Jacob, and Monod to alter their hypothesis as to the *control mechanism* of the synthesis. A year elapsed before they became seriously concerned about the relevance of the Pa-Ja-Mo experiment for *protein synthesis itself.* The irony of this situation was that as early as 1954 Pardee had carried out experiments designed to determine whether protein synthesis continues in the absence of nucleic acid synthesis, and he had come to the then surprising conclusion that "continuous formation of RNA is essential for protein formation and that the RNA found in the bacterial cell [that is, ribosomal RNA] is largely an inert metabolic product."[91]

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While for Jacob and Monod the year 1959 was crucial in the development of the messenger concept for Crick the decisive step "was taken on Good Friday (March 31, 1960) in [Sidney] Brenner's rooms in Gibbs House, King's College, Cambridge, when a number of us, including Brenner and myself, were talking to Jacob. We were asking him about this [Pa-Ja-Mo] experiment, and he was explaining that it had been improved, done in ways such that the argument was now much crisper; and at some point Brenner and I both suddenly realized the following thing: Volkin and Astrachan [working at the Oak Ridge National Laboratory] had previously shown that there was an unstable RNA in phage infection, which was DNA-like. At first they thought it was a DNA precursor, although later they got away from that idea.[92] We suddenly realized that the Volkin-Astrachan RNA was *the message*. All the experiments were done later by Brenner and his co-workers with the heavy isotope. They were planned that evening at a party in my house. Half the people were enjoying the party and half the people were in little corners trying to devise all these experiments which were later to show that messenger RNA really exists. At the same time Jim Watson and François Gros, by a quite independent line, arrived at the idea of messenger in ordinary cells . . . The only thing one is thankful for is that it wasn't all done by someone, as it were, outside the magic circle, because we would all have looked so silly. As it was, nobody realized just how silly we were."[93]

This manner of presenting the story may appear to give too much credit to the theoreticians in Cambridge, for it was Pardee who first supplied the evidence that the ribosome is not protein specific, and it was Jacob and Monod at the Pasteur Institute who were worried about the ribosome model in the light of their experiments on the switching on and off of genes. But not one of them appears to have given the unstable intermediary RNA species *exclusive preference,* and the only chemical evidence of the existence of such a class of substances (shown in Volkin and Astrachan's experiments) escaped them. But to avoid giving the wrong impression let it be emphasized that the initial concern with the ribosomal template theory and the destructive evidence came not from Cambridge but from Paris and from Harvard.

The Coding Problem

At the same time that the mechanism of information transfer from DNA to the site of protein synthesis was causing such intensive interest, the nature of the genetic code itself was a "hot" subject. In 1958 two Russian biochemists, A. N. Belozersky and A. S. Spirin, brought to a close what Crick has called the "optimistic phase" of the coding prob-

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lem (that is, how the bases in DNA determine the sequence of amino acids in proteins). Crick and Watson had already noted that the number of bases in the RNA of the tobacco mosaic virus exceeded the number of amino acids in the protein units by such an extent (50:1) that there was no clear evidence from this source as to the number of bases which code for one amino acid.[94] All that seemed likely was that some of the RNA in the virus had other functions, and was thus "nonsense" from the point of view of dictating the amino acid sequence in protein synthesis. The Russians showed that very substantial differences in the base composition of bacterial DNA was not associated with similar variations in the base composition of the bacterial RNA, though there did exist a correlation between the DNA and RNA constitution of a series of species with increasing guanine and cytosine content.[95] The messenger concept, by making ribosomal RNA nonspecific, alleviated the embarrassment to some extent. Nonetheless, cause for continuing concern was the fact that the average amino acid constitution of a group of organisms may vary little while the base composition of the DNA varies greatly. Fortunately, with the introduction of Crick's wobble hypothesis this difficulty can in theory be overcome.

Just how was the code to be broken? The direct approach of making synthetic RNA polymers, introducing them into a cell-free system and identifying the product, was not entertained at this time (1959-1960) for a variety of reasons, chiefly because the messenger concept had not been introduced. The attempt to arrive at the code by invoking stereochemical arguments about the specificity of fit between the twenty amino acids and defined sequences of bases (Gamow's idea) had been disposed of by Crick. His paper on this subject carried the following quotation on the title page: "Is there anyone so utterly lost as he that seeks a way where there is no way?"[96] Coding ideas treated in a formal mathematical manner had led to the biologically convenient and neat solution embodied in the "comma-less code," about which Crick had strong reservations.[97] With this scheme the existence of more codons (sixty-four in a triplet code of the four bases) than there are amino acids to be coded (twenty) was overcome together with the problem of how to punctuate the message by establishing twenty "sense" triplets, the remaining forty-four combinations making "nonsense."

The remaining line of attack—the attempt to demonstrate collinearity (that is, a direct relationship between the linear sequence of bases in the genetic map and the linear sequence of amino acids in the protein of the gene produce)—continued to be a very active field from the time of Seymour Benzer's classic experiments at Purdue University on the r_{II} region of phage T4.[98] The "handle" used by Brenner to open up this line of attack was mutagenesis. Exploration of the mode

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of action of certain mutagens, rather than attempts to demonstrate collinearity, led the Cambridge group on to discover that the genetic message is read from a fixed point in one direction only and that some mutagens act by addition or deletion of bases. At this time there were two ideas concerning the mechanism by which a mutant back-mutates or "reverts" to the normal "wild type." If the change which caused the mutation is simply reversed, then it is assumed that the mutation and the "back mutation" take place at the *same site* on the genetic map. If, on the other hand, the original mutation has merely been suppressed by a second mutation at a *different site* on the map, then this reversion is attributed to the action of a "suppressor." Working at Harvard, Ernest Freese discovered that the mutagens he used could be divided into two groups, no mutations induced by compounds in one group being capable of reversion by those in the other. This led Freese to support the idea of back-mutation by base substitution rather than by suppression, one class producing purine-purine and pyrimidine-pyrimidine substitutions, the other purine-pyrimidine or pyrimidine-purine substitutions.[99]

On examining earlier work carried out on the r_{II} region when Benzer had been with them in Cambridge in 1958,[100] Brenner, Leslie Barnett, and Crick found that the sites of the mutations caused by these two classes of mutagens never overlapped. This made them doubt Freese's explanation and consider another mechanism for one class of the mutagens, namely the addition and deletion of bases. The suggestion that one class of Freese's mutagens caused substitutions while the other class caused deletions and additions had been put to Freese by Bruce Alberts and his colleague Jacques Fresco, whose paper describing their model for such a mechanism appeared in 1960.[101] That January, Crick received a copy of the paper from Fresco and replied, saying that they were not very happy about the extension of the argument from synthetic RNA to DNA. "Nor do we find your discussion of mutations very illuminating. What you say is plausible enough, but it doesn't solve any of our problems, such as 'how does proflavine act as a specific mutagen?' or 'What causes hot-spots?' "[102] Crick was not satisfied with Fresco's evidence for looping-out in RNA, and was therefore loth to apply it to DNA. Today, the existence of looping-out in cellular RNAs is still unproven.

Evidently the idea of deletions and additions was foreign to their way of thinking at that time, partly because of the attractive base-change idea put forward by Freese, but also because of the presence of great differences in the mutability of different regions of the genetic map of the r_{II} region of phage T4. There are "hot spots," so-called because many of the mutations are located in them, and also regions, the deletion of which causes no impairment of function. And Fresco's scheme included no suggestions as to how the message was punctuated.

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The trouble was that no one was considering mutation from the point of view of a message which is *read from a fixed point.* Although Crick placed little reliance on the comma-less code, this theory, which did without initiator and terminator codons, had sufficient impact to restrict the ways in which investigators thought about mutagenesis. Finally, there was the overriding belief that a large proportion of the DNA is "nonsense," which meant that the odds were against a deletion at one site on the genetic map restoring the function of a gene which has undergone an addition at another site. And for those who assumed that the message is read from a fixed point there was the conviction that any deletion or addition, by causing a frame shift, would yield so much confusion as to prevent any protein being formed.

By the autumn of 1960, Brenner was driven by the data on acridines to explore a deletion-addition mechanism of mutation, and L. S. Lerman, working at the Medical Research Council Laboratory, Cambridge, in 1959 gave Brenner and Crick confidence in such a mechanism by suggesting that proflavine is occasionally intercalated between the bases of DNA on one chain only, thus answering Crick's question about how the mutagen works. Lerman thus extended the general argument of Fresco and Bruce Alberts who had shown by model building that mistakes in replication leading to mismatching of base pairs do not destroy a double RNA helix if they are looped out (see Figure 6).[103] Another paper which Fresco sent to Crick in the autumn of 1960 for submission to *Nature*[104] drew attention to the possible coding roles for looped-out segments in RNA. In Cambridge in the winter of 1960-1961 Crick and Leslie Orgel used the concept of "looping out" to devise a coding system in which messenger RNA folds back on itself to form a double helix. During translation both ascending and descending segments are then "read off" simultaneously. Paired bases could act as the code and looped-out bases as the punctuation marks or vice versa, but in either case the base composition of the *coding portion* of the molecule would be approximately the same from one organism to another although the composition of the *whole molecule* differed. Further, they noted that if base substitutions yield reversible mutations, as Fresco had suggested, then in an RNA molecule folded back on itself during translation, one would expect "a rather regular pattern of 'suppressor' mutations within a gene" (see Figure 7). "I got so excited about this," said Crick, "that I thought 'I'll do the experimental work myself,' and I started to work on the r_{II} gene in phage T4 to try and pick up internal suppressors of mutants."[105]

Starting in February 1961 he looked at quite a number of mutants but at first had no success. When he did discover the sites of some of the suppressors they did not yield the pattern predicted by the "loopy code." Early in May Crick decided to see if there were many different

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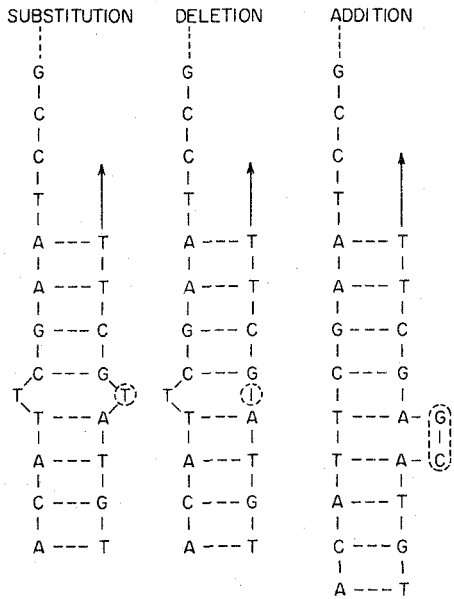

Figure 6. Hypothetical models for point mutations. In each case, the template chain is the one on the left. Note that while the templates are identical in each case, the sequence in the growing chains varies, depending on the kind of "mistake" made during replication. The "mistake" in the growing chain is indicated by a dotted circle. [The concept of "looping-out" used to suggest three types of mutation by Fresco and Alberts. (From J. Fresco and B. M. Alberts, "The Accommodation of Non-Complementary Bases in Helical Polyribonucleotides and Deoxyribonucleic Acids," *Proceedings of the National Academy of Sciences*, 46 [1960], 319.)]

suppressors. He had by this time realized that if suppressors are additions or deletions, such mutations at a number of sites could restore the wild-type function to the same mutant. The next step was to call the original mutation "plus" (a quite arbitrary notation), its suppressors "minus," to isolate the double mutants + −, and to show that they are wild or pseudo-wild (normal function restored incompletely). By the end of June, Crick was able to state that the results "are compatible with our suggestion that the action of acridines [the mutagens] is to add or delete bases. An attractive additional hypothesis is that the code is read in short groups, starting from one end of the gene. The exact starting point is supposed to determine which group is read. The deletion of a base would then alter the active reading from this point onwards. The double mutants produced by the reversion of acridine mutants would then, on this hypothesis, be altered not just in two, separated amino acids, but in a short stretch of amino acids in sequence. If this were so it would be very important for decoding."[106]

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That September Crick went to the International Biochemical Congress in Moscow, where he heard Marshall Nirenberg describe how he and Heinrich Matthaei, using the cell-free system, had discovered the synthesis of a specific polypeptide in the presence of a specific polynucleotide. Back in Cambridge Crick returned to the r_{II} mutants with renewed vigor. The double mutants were successfully isolated. Next Brenner suggested putting three pluses together—since in a triplet code the addition or deletion of three bases should restore the correct reading of the code (see Figure 8)—and Crick, realizing that it mattered whether you shifted to the right or to the left, predicted which combinations should and which should not work. What followed was reported in *Nature* at the end of the year.[107]

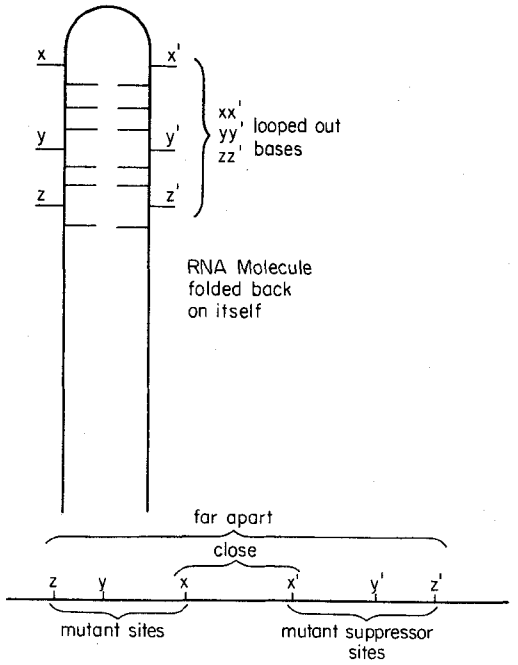

Figure 7. Loopy code scheme for suppressors.

This work, widely acclaimed as a classic piece of research, had its origin in an attempt to analyze the fine structure of the gene and to explore theories of mutagenesis. Crick, who from the beginning had played a part on the theoretical side, took up experimentation in order to test the completely erroneous hypothesis of loopy codes; he and his colleagues finished by demonstrating that the code is composed of

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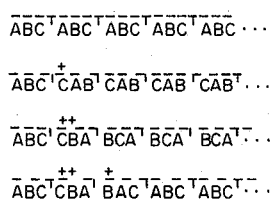

Figure 8. Diagram to show how three + mutations (assumed to be additions) restore the phased reading of the code.

triplet or multiple triplet codons, and that the message is read starting at a fixed point and traveling always in the same direction. This is a particularly striking example of how tortuous scientific research can be.

As with Watson, so with Brenner, Crick discussed ideas and plans for experiments day after day. They never collaborated in the sense of doing experiments together at the same time, and their particular research interests have been complementary, only at a very general level forming parts of the same research program. One hour's discussion of their problems per day is the rule. On such occasions they "say anything without having to justify it up to the hilt"; in this way problems get thrashed out, Crick insisting on formulating them very clearly in an analytic sense before he goes into them.[108] They interrupt each other, to continue either in dialogue or "duologue," the ideas tumbling helter-skelter from Crick to be met by a relentless questioning from Brenner. The doubts and uncertainties are explored, and they may end up with a feeling that a fresh start will have to be made. The idea was no good after all—throw it out. On this point, says Brenner, "Crick is as ruthless with himself as he is with other people."[109] In science one needs more than the ability to theorize; one must be able to relinquish theories, even neat, attractive ones like the comma-less code.

The T4 phage experiments apart, Crick's post-1953 work gives the impression that he was usually standing on the sidelines urging on the workers, interpreting their results for them and telling them what experiments to undertake next. As in the early days in Cambridge, so in the late fifties, experimentalists were not entirely happy about the intrusion of the quick mind of Crick. Some objected to his confident manner of making predictions and his insistence in letting them know when he had been proved right. Thus, speaking of Vernon Ingram's discovery that a single amino acid change in hemoglobin can prove lethal, he remarked: "It may surprise the reader . . . for my part, Ingram's result is just what I expected."[110] And again, he advanced statements such as the "wobble hypothesis" with such an air of conviction that he offended those cautious or iconoclastic members of the scientific community to

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whom generalizations are anathema. But, as Crick points out, since the professional theoretician must avoid publishing a hypothesis until he is pretty sure of it, when he has reached that state, why should he not show it?

As far as the central dogma and the sequence hypothesis are concerned, he very rightly pointed out: "It is an instructive exercise to attempt to build a useful theory without using them. One generally ends in the wilderness."[111] And on the same occasion he said: "It is remarkable that one can formulate principles such as the Sequence Hypothesis and the Central Dogma, which explain many striking facts and yet for which proof is completely lacking."[112] The sequence hypothesis states that "the specificity of a piece of nucleic acid is expressed solely by the sequence of its bases, and that this sequence is a (simple) code for the amino acid sequence of a particular protein"; the central dogma states that "once 'information' has passed into protein *it cannot get out again.*"[113] It is not possible for an alteration in protein sequence to cause a subsequent alteration in nucleic acid sequence.

These two principles—which in many popular accounts of molecular biology are referred to together as the central dogma—constitute a statement at the molecular level of the "hardness" of the hereditary material and its chemical characterization as nucleic acid and *only* nucleic acid. These statements by Crick have served to define the assumptions which were becoming implicit in a wide program of research, from mutagenesis to protein synthesis. They distinguish the Watson-Crick era from that which preceded it when nucleoproteins rather than nucleic acids were identified with the hereditary material and when ideas about the specificity of sequences were nearly all confined to those of the amino acids in proteins.

Crick's own evaluation of the role of theory is well expressed in the following passage from his 1963 paper "The Recent Excitement in the Coding Problem":

> It does not seem to be appreciated that theoretical work is often of two rather distinct types. There is first the deduction from experiment: the weighing of the data and the reasoned assessment of, say, the evidence that a particular codon represents a particular amino acid. This I would call interpretation, and it needs to be done critically.
>
> Second we have theory proper. This may take several forms; for example, Wall's demonstration that a *partially* overlapping code is not yet eliminated; or Woese's attempt to deduce the whole structure of the code from only part of it. These theories may not be correct but they are both sensible and useful, in that they enable us to tighten up our logic and make us scrutinize the experimental evidence to some purpose. Moreover, even if Woese's code is wrong, his careful exploration of its consequences may enable us to see something about the general character of the genetic code. But, most important of all, these ideas are not merely useful, they are novel. If their authors had

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not suggested them, they might not have occurred to many people working on the problem.

The bad theoretical paper takes an obvious technique, applies it to shaky data, and reaches a solution that even on general grounds can be seen to be unlikely. What is gained by this it is difficult to discover.

In the long run we do not want to *guess* the genetic code, we want to *know* what it is. It is, after all, one of the fundamental problems of biology. The time is rapidly approaching when the serious problem will be not whether, say, UUC is *likely* to stand for serine, but what evidence can we accept that establishes this beyond reasonable doubt. What, in short, constitutes proof of a codon? Whether theory can help by suggesting the general structure there is little doubt that its discovery would greatly help the experimental work. Failing that, the main use of theory may be to suggest novel forms of evidence and to sharpen critical judgement. In the final analysis it is the quality of the experimental work that will be decisive.[114]

It is clear that Crick's chief contribution to biology has been his physical sense and his ability to see through to the essence of the problem. Many ideas which were already floating around were picked up by Crick, who recognized the really important ones and made these precise, then presented them as fundamental generalizations. He had the advantage over many of his contemporaries in being self-educated in biology and biochemistry and therefore not overburdened by the trappings of the educational machine of the 1930's and 1940's. In his Cherwell Simon Lecture of 1967 he emphasized the importance to him of the ideas of symmetry and simplicity which he got from physics: these ideas, combined with a rigorous examination of the evidence, made it possible for him to see quickly the fallacies in theories of the code based on the amino acid specificity of holes in the RNA helix. Later he exposed the errors in the work of S. R. Pelc and M. G. E. Welton, purporting to show that amino acids fit their transfer RNA codons specifically. When Pelc showed Crick his models, Crick found that all the polynucleotide sequences had been built backwards.[115]

Recently Crick has turned his attention to diffusion models as mechanisms for differentiation. In typical fashion he has picked out the significant fact, already noted by Lewis Wolperts, that embryonic fields appear to involve only short distances of up to one hundred cells, and he is examining a scheme involving facilitated diffusion between the members of a two-dimensional sheet of cells, of the kind found in the insect cuticle, which his postdoctoral student, Peter Lawrence, is studying in insect cuticle. Crick's recent paper in *Nature* on the subject could only have been written by him: the style of the arguments, the frequent recourse to quantitative data, and the confident predictions ("it is my belief that mechanisms based on diffusion are not only plausible but rather probable") are unmistakable.[116]

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Conclusion

It is now seventeen years since the discovery of the Watson-Crick model for DNA. Crick is now codirector (with Brenner) of the Cell Biology Division of the Medical Research Council Laboratory with thirty-three research graduates under him, some doctoral and some postdoctoral. In the old days he had very little correspondence, no administrative duties, and he deliberately avoided teaching. At first this was because he wanted to get his Ph.D. thesis completed as soon as possible. Later when the opportunity for graduate teaching came again, he no longer felt a need for the money. At University College, London, he had done his spell of demonstrating (laboratory supervision and instruction); at Cambridge he was happy to be free from it. In those days his journey to the lab at the Cavendish was only a short walk; there he worked long hours, including Saturday mornings and often Saturday afternoons. After lunches at the Eagle there was always a walk along the "Backs," when many an animated discussion took place. Now he drives to work at the new lab in Hills Road, three miles from the center of the town. He has a sizable correspondence and much of his time goes in deciding the research program of the division and in finding the right people to work on it (most of the research staff are appointed for three- to five-year terms).

Crick himself is trying to escape from the domination of his life by science. He gets through much more work in less time, yet feels uncomfortable about the delegation of the work to others. (One has to get used to the idea of enjoying in a vicarious manner the progress of the work in the hands of a younger man.) Of course his style has always been toward the theoretical rather than the practical side, but it would be a mistake to regard model building, for instance, as not deserving the label "experimental," for with models one manipulates the environment of the molecule and observes the result. The model is used as an analogue computer. It is often assumed that bench work is "work" and thinking is not. Here again, Crick would argue that thinking, discussing, and communicating are all work, and they all take time. Nor is Crick satisfied with the formal correspondence of a mathematical model with biological reality; for him a physical and chemical reality must also be achieved. The "black box" situation in biology, though of interest to Crick as the starting point of an investigation, has always been for him a preliminary to discovering the machinery within. No model for pattern formation during differentiation will satisfy him until an actual "morphogen" has been isolated and shown to possess the required properties.

Many theoreticians are less concerned about the experimental evidence than Crick. At the same time Crick's demands for evidence did

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prevent him from publishing his adaptor hypothesis when he first thought of it in 1955, which was, on reflection, a pity.

At a time when molecular biology is being attacked as too narrow and when the trumpet call is for hierarchical laws, Crick remains undisturbed. He doubts that if these critics knew more chemistry they would maintain their opposition. The place of DNA in biology may be challenged, but such threats are due more to misunderstanding and misrepresentation of the claims of the subject than to the program of research as such. When Michael Polanyi states that vitalism, as he defines it, "can be ignored only by a truculently bigotted mechanistic outlook,"[117] it is clear that he is concerned with rather different matters than the molecular biologist, who would not claim that a piece of Shakespeare's writing is reducible to physics and chemistry though he would claim that the thought processes which generate them will be explicable in terms of those sciences. Meanwhile, evidence for the structure of DNA, the sequence hypothesis, and the central dogma is accumulating. Some facts—reiterated DNA sequences, for instance[118]—do not appear at first sight to square with the theory; but the weight of such evidence is unclear until more is known about it.

Finally, we may try to predict how this body of theory will look at the end of the century. Will it have lost some of its charisma, its public appeal? Clearly the latter quality stems partly from its medical importance, just as does the Curies' discovery of radium. Will the DNA discovery be looked upon as a transforming principle? Undoubtedly, the connection it has with the work that preceded it will become clearer with the passage of time, but what Gardner Quarton called the "psychological factors associated with a transforming principle"[119] will remain; to anyone who goes back over the pre-1950 literature the contrast in outlook is very striking, for it was in the 1950's that DNA rose to a special position of importance due to the work of Erwin Chargaff, the phage biologists, and the Watson-Crick structure for DNA.

That the reproduction of the gene could be based on so simple an idea as complementary hydrogen bonding was almost too good to be true, and this caught the imagination of scientists, funding institutions, the Nobel Prize Committee, and, finally, the general public. The discovery will also retain a certain aura, due to Watson's book *The Double Helix*.

From the technically oriented physics at the Admiralty, Crick has moved to X-ray crystallography and on to genetics, protein synthesis, and now to differentiation. "He is," to quote Brenner, "the only one in the MRC Lab at Cambridge who can encompass all the work there even at a very deep level . . . He is a very great force there." His career is proof of the fact "one learns the field best by helping to create it."[120]

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This year Crick stated confidently that "nature usually has such difficulty evolving elaborate biochemical mechanisms (for example, those used in protein synthesis) that the underlying processes are often rather simple."[121] The physicist in Crick is still hard at work, and molecular biology is by no means an exhausted subject.

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1. The source material for this account consists of published papers, unpublished correspondence and papers, and a number of oral interviews and informal conversations. Extracts from taped, oral interviews are referred to as interviews and are dated, whereas informal conversations by telephone and in person are simply described as personal communications and are undated. Those readers who desire more explanation of the subject matter of molecular biology may like to consult J. C. Kendrew's lucid book *The Thread of Life* (London: Bell, 1966). They will also find in Gunther Stent's essay on DNA in this issue of *Dædalus* a very clear exposition of the subject of molecular genetics.

   It is a pleasure to express my thanks to all those who have helped me in collecting the material for this essay, and especially to Dr. and Mrs. Francis Crick, Dr. Sidney Brenner, Mrs. Arnold Dickens of Northampton, and Mrs. Anne Sanderson of the Medical Research Council, London. I am also grateful to Dr. J. D. Watson for reading this paper. This work has been carried out with support from the Royal Society and the American Philosophical Society.

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